Human Behaviour and the Environment: Experiments in Behavioural Environmental Economics

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Declaration

This thesis is submitted according to the requirements of the Degree Committee of Land Economy. It does not exceed the regulation length of 80,000 words including footnotes, references and appendices. It is the result of my own work and includes nothing which is the outcome of work done in collaboration with others, except where specifically indicated in the text and Acknowledgements.

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Preface

The work within Chapter 1 is based on a collaboration with Dr. Elisabeth Gsottbauer (University of Innsbruck) and Prof. Jing You (Renmin University of China).

The work within Chapter 3 is based on a collaboration with Dr. Elisabeth Gsottbauer (University of Innsbruck) and Dr. Sander van der Linden (University of Cambridge).

The work within Chapter 4 is based on a collaboration with Dr. Elisabeth Gsottbauer (University of Innsbruck) and Anya Doherty (Foodsteps Ltd.).

Prof. Andreas Kontoleon was involved in the conceptualisation of each of the chapters in this thesis. The above-mentioned collaborators assisted with the conceptualisation, methodological queries, implementation and review of the research. For all chapters, I have led the research and remain first author in the papers produced from them. I was also solely responsible for the literature review, methodology, investigation, data management and manipulation, quantitative analysis, visualisation, writing of the original draft and the editing process.

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I dedicate this thesis to my loving parents. To my father who set me on the academic path and has always supported me to achieve my full potential. And to the loving memory of my mother, who I know would have been proud of me, if she were still here with us today.

Abstract

Human behaviour lies at the heart of the climate crisis. Not only is it the primary cause of global climate change and environmental degradation, but also key to responding and adapting to them. Tackling the climate crisis thus requires a complete understanding of human behaviour. So far, however, environmental policies have largely been guided by the canonical economic model of human behaviour, based on the idea of 'homo economicus', neglecting important behavioural aspects of the relationship between human behaviour and the environment.

This thesis examines some of the complex interrelations between human behaviour and the environment through a behavioural environmental economics lens, drawing on recent insights from behavioural economics and psychology. The first part of this thesis (Chapters 1 and 2) focuses on the impact of environmental stressors on human behaviour, attitudes and beliefs. The second part (Chapters 3 and 4) examines the impact of policy interventions to foster more environmentally sustainable behaviour. All chapters adopt an experimental or quasi-experimental approach to provide causal insights and formulate robust policy recommendations.

Chapter 1 develops and tests a novel experimental design, that exploits natural discontinuities in air pollution episodes in Beijing, China to experimentally isolate the causal effect of acute air pollution on social decision-making and economic preferences. Chapter 2 utilises data from a natural experiment to estimate the causal effect of extreme weather events on climate change attitudes and pro-environmental behaviours. Chapter 3 uses an online 'message framing' experiment to explore whether appealing to 'warm glow' motives can encourage voluntary pro-environmental behaviour, relative to other common climate change communication strategies. Chapter 4 presents the results of a large-scale field experiment conducted at five university cafeterias, exploring whether carbon footprint labels can promote more sustainable food choices.

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Introduction

Climate change is the most pressing global challenge facing humanity in the 21st Century. The latest assessment of climate science from the Intergovernmental Panel on Climate Change (IPCC), synthesising work from over 14,000 peer-reviewed studies, establishes the "unequivocal" influence that humans have had on the warming of the atmosphere, ocean and land (IPCC, 2021). However, human impact on the environment reaches far beyond global warming. Since the industrial revolution, environmental degradation and the emissions of dangerous pollutants into the atmosphere have accelerated at an unprecedented rate, contributing to the decline of societal health and the destruction of biodiversity (Ceballos et al., 2015; Lee & Greenstone, 2021). Humanity's footprint on the planet is now so evident that some scientists have proposed the onset of a new geological epoch - the Anthropocene (Crutzen, 2002; Waters et al., 2016).

Undoubtedly, the growing academic knowledge base and the increasingly tangible impacts of climate change have shifted climate change mitigation and adaptation to the top of the policy agenda in many countries across the globe. Moreover, it is increasingly being recognised that human society is vitally dependent on healthy natural or semi-natural ecosystems (Díaz et al., 2015). The tangible health and mortality impacts of air pollution have stimulated ambitious policy initiatives to curb polluting activities (Greenstone et al., 2021). However, further transformational changes in society are necessary to achieve rapid global decarbonisation and stabilise the climate (Otto et al., 2020; Perlaviciute et al., 2021). At the core of this transformational challenge lies the interrelation between human behaviour and the environment. While the impact of environmental change on society is increasingly well understood, crucial questions remain unanswered with respect to reducing society's impact on the environment.

A long tradition of research in the field of environmental and resource economics has emphasised the role of market failures as the root cause of many of today's environmental challenges (Arrow, 1969). Environmental market failure – a departure from a Pareto efficient market system – can be grouped into the taxonomy of externalities, nonrival goods, non-excludable benefits and costs, nonconvexities, and asymmetric information (Shogren & Taylor, 2008). The social inefficiencies arising from environmental market failures thus warrant government intervention via environmental policy. Traditional environmental policy tools include Pigouvian taxes and subsidies, tradeable permit systems or command-and-control regulation. The view that market-based policy interventions can solve market failures is firmly embedded in the neoclassic economic paradigm of rational, self-interested behaviour and stable preferences. However, the neoclassic model of "homo economicus" does not take into account important aspects of human behaviour and its interaction with the environment.

The rapidly emerging field of behavioural environmental economics extends the traditional neoclassical framework by integrating more realistic models of human behaviour, including bounded rationality, bounded willpower, and bounded self-control (Mullainathan & Thaler, 2000). The behavioural economic approach, influenced most notably by the ground-breaking work on behavioural anomalies (Kahneman, 1979; Tversky & Kahneman, 1974), departures from self-interest (Fehr & Fischbacher, 2003; Fehr & Schmidt, 1999) and libertarian paternalism (Thaler & Sunstein, 2003) is increasingly recognised as the "new normal" economic approach and has laid the foundations for the emerging field of Behavioural Public Policy (Oliver, 2013; Reisch, 2021). In this regard, several recent reviews have advocated for the incorporation of behavioural-economic approaches into environmental policy (Croson & Treich, 2014; Kesternich et al., 2017; Shogren & Taylor, 2008). Shogren and Taylor (2008) argue that robust environmental policy interventions must take into consideration "behavioural failures" (i.e., behavioural biases and heuristics) which exist simultaneously and interact with traditional "market failures".

Moreover, a recent increase in the use of experiments in environmental economic research has been critical in advancing our understanding of human behaviour in relation to environmental policy (Bouma, 2021). While lab and choice experiments can provide important foundations for policy evaluation, Randomised Controlled Trials (RCTs) have become the primary method for causal identification and promise greater external validity when applied in field settings (List & Price, 2016). Taken together, recent advances in behavioural environmental economics have seen a renewed focus on empirically valid models of human behaviour which imply a holistic view of how people think and behave within socio-ecological decision contexts. Moreover, behavioural environmental economics has embraced an evidence-based economic approach, based on the method of experimentation, to bring causal (behavioural) insights to current academic and policy disputes regarding environmental issues.

Within this context, the overarching objective of this thesis is to examine some of the complex interrelations between human behaviour and the environment through a behavioural environmental economics lens. The research presented contributes to the academic and policy debates in specific under-explored research areas. The thesis is split into two parts: Part 1 strengthens our understanding of the impacts associated with climate change and environmental degradation on human behaviour, attitudes and beliefs. Part 2 experimentally explores multiple behaviour change strategies to encourage pro-environmental behaviour and thereby adds to our understanding of how behaviours can be influenced to lessen the anthropogenic impact on the environment. The findings from both parts have important implications for evidence-based transformational policies to accelerate a transition towards a more sustainable future.

The research presented in this thesis takes an interdisciplinary approach by combining methodologies from behavioural and experimental economics, environmental psychology, and empirical microeconomics. Each chapter adds a unique methodological dimension – utilising experimental and quasi-experimental techniques – building on work defining a new era of applied economic research which has been recognised with multiple recent Nobel Memorial Prizes.[1] A common feature across all four chapters is the use of random or quasi-random assignment, allowing a causal interpretation of the research findings. Moreover, an important aim of this thesis is to attain high standards for research transparency and reproducibility (Christensen & Miguel, 2018). The experimental studies are therefore based on carefully developed pre-analysis plans, two of which were pre-registered on open-science platforms prior to data collection. The thesis consists of four empirical chapters, split into two parts, which are summarised below.

Part 1: Environmental stressors - implications for human behaviour

The first part of this thesis focuses on the impact of climate change and environmental degradation on human behaviour. It is founded in the important recognition that human decision-making, preferences, and beliefs are context-dependent, influenced, in part, by the socio-ecological decision contexts. Specifically, the chapters in the first part explore the role of air pollution and extreme weather events in shaping human behaviour, preferences

[1]Specifically, the research draws on contributions by Abhijit Banerjee, Esther Duflo and Michael Kremer who brought the field-experimental approach to applied economic research, Richard H. Thaler, a pioneer in behavioural economics, and most recently Joshua D. Angrist and Guido W. Imbens whose methodological contributions provide the foundation for parts of the analysis presented in this thesis.

and beliefs. Both topics have received considerable interdisciplinary interest. In economics, the research places within a broader literature which primarily employs quasi-experimental methods to study the impact of environmental stressors on a range of economic and social outcomes (Burke et al., 2016; Carleton & Hsiang, 2016; Dell et al., 2014; Kolstad & Moore, 2020). In environmental psychology and behavioural science, there has been a particular interest in the relationship between personal experience of climate change (e.g., via environmental stressors), risk perceptions and engagement with climate change (van der Linden, 2014, 2015b). The research presented in the first part contributes to our empirically grounded understanding of how human behaviour responds to environmental stressors and makes important methodological contributions. A detailed summary of the motivations and main contributions are provided below.

Chapter 1: The causal effect of air pollution on anti-social behaviour

Air pollution poses the greatest global threat to human health and well-being. According to the World Health Organisation (WHO), in 2019, more than 90% of the world's population lived in areas in which air pollution concentrations exceeded thresholds considered as safe (WHO, 2021). While the physiological health impacts of long-run air pollution exposure are well studied (e.g. Pope et al., 2020; Schraufnagel et al., 2019), an emerging literature in economics examines the "hidden impacts" of air pollution (Zivin & Neidell, 2018). This literature has provided evidence that exposure to air pollution is associated with a myriad of psychological, economic and social impacts (Lu et al., 2020). Amongst these, less is known about how air pollution exposure affects anti-social behaviour. Yet, understanding what determines and undermines social behaviour has important implications for how to address major societal challenges, such as climate change or public health crises (Van Lange & Rand, 2022). In addition, a long-standing challenge in this field of research has been the identification of causal relationships. While RCTs would provide the strongest form of causal evidence, randomly exposing people to extreme levels of air pollution (vs. no pollution) in an experimental setting would be practically challenging and ethically questionable. Most of the existing research has therefore relied on quasi-experimental designs and proxy measures of anti-social behaviour (e.g., crime statistics).

To address these shortcomings, the first chapter develops a novel experimental design, which combines elements of a lab-in-the-field experiment with online data collection procedures, to experimentally examine the causal effect of acute air pollution on anti-social behaviour.[2] It improves on previous research by measuring anti-social behaviour using a set of well-established incentivised economic games, specifically selected to study fundamental aspects of social decision-making, independent of contextual factors. The experimental design randomly assigns participants to be surveyed on high and low-pollution days, exploiting rapidly occurring natural discontinuities in air pollution episodes. Data were collected in real-time using social media channels (i.e., WeChat Messenger) to contact participants during high and low pollution episodes. The methodology thus builds on the 'Experience Sampling Method' used in subjective well-being research (Fujiwara et al., 2017; MacKerron & Mourato, 2013) but additionally introduces experimental identification using a stratified randomisation procedure. Furthermore, it extends this literature by utilising popular social media channels to collect data. In sum, the research presented in Chapter 1 pushes the boundaries on how experimentation can be used to study the impact of external environmental stressors (Almås et al., 2019; Dean, 2019) and is the first to focus on air pollution.

Chapter 2: Does flood and heatwave experience shape climate opinion? Causal evidence from flooding and heatwaves in England and Wales.

Although the planet is unambiguously warming, the psychological distance of climate change and the uncertainty around future costs have often served to justify political inaction and delay mitigation initiatives (Spence et al., 2012). However, in recent years, climate change impacts are becoming increasingly evident at the local level, manifested by changes in the frequency and intensity of extreme weather events (IPCC, 2021). Therefore, it has become a priority of applied social science research to understand how personal experiences with extreme weather events shape mitigation and adaptation decisions at the individual level (Howe, 2021; Sisco, 2021). More specifically, it has been hypothesised that personally experiencing extreme weather events will reduce the psychological distance of climate change and lead people to update their beliefs via experiential learning.

The interdisciplinary interest in this topic, extending across economics, psychology and sociology, has produced a large body of evidence, drawing on a range of methodologies which has provided largely inconsistent results (Howe, 2021). The bulk of this literature has primarily relied on inferior correlational research designs using data at spatially aggregated scales (e.g., at the state level). The lack of methodological quality limits the internal and

[2] Gneezy and Imas (2017, p.439) define a lab-in-the-field experiment as a method that "combines elements of both lab and field experiments in using standardised, validated paradigms from the lab in targeting relevant populations in naturalistic settings".

external validity of the existing body of evidence. While the majority of studies establish whether a significant effect on beliefs or behaviour can be identified, little is known as to when and how personal experience changes beliefs and behaviour (Brügger et al., 2021). Moreover, there is limited research on how individual-level pro-environmental behaviour is affected by extreme weather events, which is crucial to our understanding of adaptive processes.

Chapter 2 exploits geo-referenced panel data on climate change attitudes as well as natural variation in flood and heatwave exposure to estimate the causal effect of extreme weather events on climate change attitudes and environmental behaviours. The study utilises climate change opinion data from the 'secure access' version of the UK Household Longitudinal Survey (UKHLS), which provides geo-referenced location information for each survey participant (University of Essex, Institute for Social and Economic Research [University of Essex], 2020).[3] In doing so, it allows a spatially precise identification of flooding and heatwave exposure using Geographic Information Systems (GIS). Moreover, the study significantly improves on previous research by utilising a generalised difference-in-differences methodology combined with a propensity score matching approach to minimise potential selection bias from residential sorting. Chapter 2 is the most methodological rigorous analysis to date, and one of the first to provide causal insights into when and how personal experience with extreme weather events influences climate change attitudes and behaviour.

Part 2: Policy interventions for pro-environmental behaviour

The second part of this thesis examines policy interventions aimed at reducing environmental externalities from human behaviour. Understanding how to encourage wide-scale persistent behaviour change remains a major priority for research and policy (Steg, 2018). Individual behaviour can be influenced through a range of policy instruments including conventional measures (such as taxes and regulation), education and information provision as well as behavioural interventions. The chapters in Part 2 focus on the latter two strategies. In recent years, behavioural interventions have received much attention as an environmental policy tool, as they promise to be relatively inexpensive, easy to implement and less intrusive than conventional policy measures (Carlsson et al., 2021; Gravert & Olsson, 2021). Nevertheless, information provision continues to play an important role in mobilising sustainable consumer behaviour (Thøgersen, 2021). The research presented in Part 2 draws on experimental methods to provide causal insights into the efficacy of different behaviour-change strategies and is

[3] Access was granted following specialised training and accreditation as an ESRC Accredited Researcher.

situated within the broader applied impact evaluation literature focussing on environmental policy instruments (Byerly et al., 2018). It adds to the interdisciplinary literature examining the efficacy of behavioural interventions to encourage voluntary pro-environmental behaviour (Velez & Moros, 2021) and contributes to the growing body of work on sustainable consumer behaviour (Thøgersen, 2021). A detailed summary of the motivations and main contributions are provided below.

Chapter 3: Turning up the heat: Encouraging pro-environmental behaviour through warm glow

Many people are intrinsically motivated to act pro-environmentally (Steg, 2016). On the one hand, intrinsic motivation may stem from the personal desire to act morally (i.e., in line with one's values) and in turn avoid negative moral emotions such as guilt and shame. On the other hand, behaviours may be intrinsically motivated because helping the environment makes people feel good about themselves. In the economics literature, the emotional reward from pro-social behaviour has been termed 'warm glow', which is the focus of Chapter 3. While the importance of intrinsic motivation is well-established, many existing policy approaches for sustainable behaviour change have relied on providing external incentives, such as financial rewards or social recognition. Although such extrinsic motivators may be successful in the short-term, they usually fall short of achieving persistent behaviour change (van der Linden, 2015a). Therefore, it remains an important priority to understand how intrinsic motivations can be harnessed to achieve long-run behaviour change and guide habit formation.

Chapter 3 proposes that directly appealing to 'warm glow motives' may be a promising approach to harness intrinsic motivation towards the environment. It presents one of the first attempts to experimentally manipulate and measure warm glow experiences in the context of pro-environmental behaviour. To do so, it utilises a large-scale online 'message framing' experiment which randomly assigned participants to receive either a 'warm glow' appeal or a message informed by other typical communication strategies, including guilt framing, social norm messaging and the provision of basic information. It improves on previous lab-based research by introducing a novel real-effort task to measure incentive-compatible pro-environmental behaviour in an online experimental setting. It capitalises on the strengths of a controlled experimental setting to measure anticipated and experienced emotions, which are explored as potential mechanisms. The research presented in Chapter 3 is one of the first explorations into environmental warm glow as an intrinsic motivator of voluntary climate action, and identifies priorities for future research.

Chapter 4: Do carbon footprint labels promote climatarian diets? Evidence from a large-scale field experiment

Emissions from the food system are responsible for up to a third of global greenhouse gas emissions (Crippa et al., 2021), with a disproportionally large share arising from animal-based foods (Xu et al., 2021). Reducing consumption of high-carbon impact foods, thus, presents one of the single most impactful pro-environmental behaviours that consumers can pursue (Reisch, 2021). In recent years, more and more people actively follow diets that aim to reduce their carbon footprint from food consumption, so called 'climatarian diets'. However, consumers often lack knowledge of the carbon footprint of different types of food and tend to underestimate their environmental impact (Camilleri et al., 2019). Carbon footprint labels present a viable policy tool to address the market failure of information asymmetry and lack of full information on the environmental externalities associated with food production and consumption (Asioli et al., 2020). While carbon footprint labels have shown their potential to reduce emissions in other domains (e.g., on energy appliances), robust evidence on their ability to shape food choices is largely lacking today. The existing literature has primarily employed stated preference techniques or relied on other hypothetical food choice contexts.

Chapter 4 presents the results of a large-scale field experiment conducted at five university cafeterias to test whether carbon footprint labelling can encourage more sustainable food choices. Prior to the implementation, carbon footprints were calculated for 500 unique cafeteria main meals and a carbon footprint label was developed and pre-tested in collaboration with industry and catering stakeholders, as well as professional graphic designers. The experiment was conducted over a period of six months and tracked the food choices of over 2,600 individuals, resulting in a large dataset of 85,000 individual purchase decisions. It extends previous research by utilising a differences-in-differences approach to account for potential time-confounding factors, which allows a causal interpretation of the findings. A supplementary analysis links individual-level food choices with exit survey data, collected after the intervention, to explore attitudes towards the labels and potential mechanisms driving changes in behaviour. In sum, the research presented in Chapter 4 is the first large-scale assessment of carbon footprint labels on food and one of the first to provide tentative insights into potential mechanisms. Its findings may directly inform the concurrent policy debate on sustainable food policy (Reisch et al., 2021).

Synthesis and thesis structure

Taken together, the four empirical chapters presented in this thesis aim to provide causal insights into some of the complex relationships between human behaviour and the environment. A common starting point for each of the four distinct, yet connected, chapters is the recognition that environmental policy must be guided by empirically valid models of human behaviour, informed by recent developments in behavioural economics and psychology. The first part of the thesis takes into account that human behaviour is actively shaped by the socio-ecological contexts in which decisions are made (Biggs et al., 2021). Chapters 1 and 2 thus explore how exposure to environmental stressors influences behaviour along multiple dimensions. The second part of the thesis acknowledges that decision-making is actively influenced by the behaviour of others and driven by internal emotions (Lerner et al., 2015; Nyborg, 2018). To that end, Chapters 3 and 4 draw on experimental methods to test several environmental policy instruments, which either seek to harness intrinsic and extrinsic motivators, or correct information asymmetries to allow consumers to make more sustainable choices.

An overlapping theme discussed in Chapters 2, 3 and 4 is that of pro-environmental behaviour (PEB). The focus on PEB addresses the urgent need for research to systematically identify "effective and acceptable ways to encourage people across the world to consistently engage in climate mitigation actions so that their collective efforts will limit climate change to 1.5°C" (Steg, 2018, p.761). To that end, Chapters 3 and 4 test intervention strategies specifically aimed at stimulating PEB, whereas Chapter 2 explores whether personally experiencing the impacts of climate change may in itself encourage the uptake of PEB. Moreover, Chapter 1 focuses on the stability of social behaviour under environmental stress, which arguably serves as an important precondition for collective action on climate change.

In addition to overlapping themes, this thesis also draws on multiple overlapping methodologies from the behavioural environmental economics toolkit (Bouma, 2021). Chapters 1, 3 and 4 utilise controlled experimentation to uncover causal relationships and develop robust policy recommendations. Chapter 2 exploits data from a natural experiment. "Controlled" experiments, conducted in laboratory or field settings, "represent the most convincing method of creating the counterfactual, since they directly construct a control group via randomization" (Harrison & List, 2004, p.1014). Each chapter capitalizes on the relative strengths of different experimental techniques – online, lab-in-the-field, field, and natural experiment – to match the respective research context.

Not only does this thesis utilise state-of-the-art methods, but it also seeks to advance how experimentation can be used to study human behaviour. To that end, Chapter 1 combines elements of a lab-in-the-field experiment (i.e., targeting relevant populations in naturalistic settings) with online data collection via social media channels to collect data at pre-specified times. The approach outlined in Chapter 1 provides a blueprint for future data collection procedures – in near real-time and based on randomised designs. Developing innovative approaches to large-scale data collection incorporating experimental design features will play an important role in addressing concerns regarding the external validity and lack of scalability of experimental research findings (Al-Ubaydli et al., 2021).

Nonetheless, it is important to acknowledge that not all research questions lend themselves to experimentation in a field or lab-in-the-field setting. To that end, laboratory experiments will continue to play an important part in studying human behaviour, which will increasingly be conducted in "virtual settings" over the internet. To address the apparent need for online experimentation, Chapter 3 introduces a novel experimental measure of pro-environmental behaviour which can be implemented in online experiments. Chapter 3 is exemplary of how online participant pools can be used to conduct large-scale longitudinal online RCTs with minimal attrition.

Each of the four chapters is written as a stand-alone paper, including an introduction and literature synthesis specific to the topic of the chapter. Moreover, conclusions and policy recommendations are formulated at the end of each chapter. Each chapter has its own appendices (labelled in alphabetical order) and bibliography. Appendices contain additional robustness checks, tables and figures, and experimental protocols. The thesis can be read in any order; however, the suggested order corresponds to the conceptual reflections made in this introduction.

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Chapter 1

The causal effect of air pollution on anti-social behaviour

1.1 Introduction

Air pollution is a growing global health concern, and its adverse and in many instances lethal effects are widely documented (Arceo et al., 2016; Currie & Neidell, 2005; Kampa & Castanas, 2008; Lelieveld et al., 2015; Pope et al., 2020; Pope et al., 1995; Schlenker & Walker, 2016). In addition to acute harm, usually manifested by respiratory or cardiac symptoms, air pollution potentially harms every organ in the body, including the brain (Schraufnagel et al., 2019). An emerging literature in economics shows that air pollution is associated with a range of economic and behavioural outcomes (Zivin & Neidell, 2018). For instance, air pollution has been found to adversely impact human capital formation, including worker and firm productivity and educational outcomes (T. Chang et al., 2016; T. Y. Chang et al., 2019; Currie et al., 2009; Ebenstein et al., 2019; He et al., 2019; Heyes et al., 2019; Zivin & Neidell, 2018, 2012). Another area of concern is that air pollution appears to cause more aggressive and violent behaviour. Numerous studies have shown that higher daily air pollution levels are positively associated with observed criminal activity, suggesting a direct link between pollution exposure and anti-social behaviour (Bondy et al., 2020; Burkhardt et al., 2019; Herrnstadt et al., 2021; Lu et al., 2020, 2018; Zou, 2021). Exploring this relationship is the central focus of this chapter, whereby we address two shortcomings of the existing literature.

Firstly, while some studies have documented a significant relationship between pollution and measures of pro and anti-social behaviour, a long-standing challenge has been to isolate potential mechanisms. Both physiological and psychological pathways may be at play. Recent advances in neurotoxicology suggest a direct translocation pathway: due to their small size, ultrafine particulate matter smaller than 2.5 µm (PM2.5) are able to directly enter the brain via the nasal mucosa or by crossing the blood-brain-barrier (Boda et al., 2020; Kilian & Kitazawa, 2018; Power et al., 2016; Thomson, 2019). This in turn can trigger oxidative stress and systemic inflammation in the brain, which raises stress hormone levels (Costa et al., 2014; H. Li et al., 2017). If these direct physiological channels are at play, one may plausibly expect to observe a positive relationship between air pollution and anti-social behaviour, yet there may be other, indirect and potentially interrelated pathways that explain the relationship. One possible explanation is that air pollution impacts cognitive ability, thus resulting in suboptimal choices (Archsmith et al., 2018; S. Chen, Guo, et al., 2018; Lai et al., 2021; Shehab & Pope, 2019; Steffen et al., 2019; Zhang et al., 2018). Others have suggested that air pollution may change the way people think about the future by temporarily increasing the discount rate applied to intertemporal trade-offs (Bondy et al., 2020). Increased discounting, potentially linked to elevated stress hormone levels, would imply that individuals place greater relative value on short-term gains and underestimate the potential of future punishment, which is consistent with an increase in criminal behaviour. However, exposure to air pollution may also increase risk aversion (Chew et al., 2021; Heyes et al., 2016), which in turn should lead to less criminal activity (Bondy et al., 2020). The psychological literature posits that air pollution might affect decision-making because of its negative effect on emotional well-being, state anxiety and mental health (S. Chen, Oliva, et al., 2018; Khan et al., 2019; Y. Li et al., 2019; Lu et al., 2020; Sass et al., 2017; Zeng et al., 2019; Zhang et al., 2017). It has also been argued that elevated pollution levels may increase sleeplessness, which in turn might be linked to changes in decision-making (Heyes & Zhu, 2019).

A second challenge in the existing literature exploring the impact of pollution on social behaviour is the identification of a causal effect. The vast majority of previous literature has relied on ex-post analyses employing quasi-experimental research designs such as panel regression or instrumental variable analysis (Lu et al., 2020). While quasi-experimental methods allow for a causal interpretation of the results, the findings are sensitive to assumptions of identification and sources of variations and therefore, may not achieve the same degree of internal validity as results obtained from a controlled experimental setting.

In contrast to the quasi-experimental setting, lab experiments allow exogenous manipulation of the degree of air pollution to which participants are exposed. While similar studies have been conducted with respect to other environmental stressors, including thermal stress (Almås et al., 2019) and noise pollution (Dean, 2019), exposing individuals to high levels of air pollution would entail serious ethical concerns and potential health impacts. Nonetheless, attempts have been made to experimentally manipulate the level of air pollution exposure, for instance by burning candles in the study space (Shehab & Pope, 2019), by priming participants with vivid imagery of clean versus polluted cities (Lu et al., 2018), by providing sham air purifiers for student dorm rooms (H. Li et al., 2017) or by exposing individuals to diesel exhaust (Crüts et al., 2008). However, the prior two cannot guarantee sufficiently large variation in exposure (or perceived exposure) to air pollution and the latter two are ethically questionable.

More recently, emerging research has linked air pollution measurements with behavioural outcomes obtained from "natural laboratory settings" matching individuals' experimental data from the lab (or similar controlled settings) to the air quality of the survey day (Bedi et al., 2021; Chew et al., 2021). This allows for both stronger identification of exogenous changes of pollution to individual behaviour – without the aforementioned ethical concerns – and direct investigation into transmission channels. The closest study to ours is Chew et al. (2021) who use data from a set of incentivised laboratory experiments measuring economic and social decision-making, which were collected during and after an extreme pollution episode in 2012. However, none of the existing "natural laboratory experiments" were specifically designed and pre-registered for the purpose of assessing the impact of air pollution on (anti-)social behaviour and other outcomes. To that end, subjects' exposure to air pollution was not randomised, but rather depended on self-selection into experimental time slots, which gives rise to potential endogenous sorting on anticipation of air pollution. In this regard, the existing "natural laboratory" research also falls into the quasi-experimental identification strategy.

In this study, our goal is to advance economic experimental research of the effects of air pollution on social behaviour by introducing a novel experimental design that addresses both the aforementioned challenges.[1] Firstly, alongside the primary outcomes of interest (anti-social behaviour and norm-enforcement), we measured a range of secondary outcomes, which may constitute potential mechanisms. We used incentivised experimental tasks to measure cognitive ability, risk and time preferences. We also used a range of verified scales and self-report questions to assess depression, momentary emotions, and self-control depletion. Secondly, our pre-registered study was specifically designed to exploit naturally occurring discontinuity in pollution and measure social and economic preferences on both high and low-pollution days in Beijing, China during the winter of 2019. Based on a set of criteria,

[1]Ethical approval for the experiment was granted by the Department of Land Economy Ethical Research Committee. The study was pre-registered at the AEA RCT Registry, October 2019. https://doi.org/10.1257/rct.4856-1.3000000000000003.

we proactively selected high and low-pollution days on which our survey was administered via WeChat to a sample of 632 university students. Our randomised online experiment, thus, allows for greater experimental control, a higher degree of identification and utilises a set of well-established incentivised economic games to measure a range of social and economic preferences.

Our experiment makes multiple contributions to several strands of economic literature. From a methodological standpoint, we contribute to an emerging literature which utilises controlled experimental settings to explore the impact of environmental stressors on economic behavioural measurers (Almås et al., 2019; Bedi et al., 2021; Chew et al., 2021; Dean, 2019). We extend this literature by introducing a novel experimental design that combines elements of a lab-in-the-field experiment with online data collection procedures via social media channels (Gneezy & Imas, 2017). The experimental design exploits naturally occurring discontinuities in air pollution to mimic an experimental setting in which air pollution exposure is exogenously manipulated. Individuals were randomly assigned to either low or high-pollution exposure, which provides clean experimental variation and hence allows us to identify a causal effect. Our novel methodology, thus, speaks to the broader empirical literature that aims to isolate the effects of air pollution on social and economic decision-making (Lu, 2019; Zivin & Neidell, 2018).

In addition to methodological advancements, the findings from this study contribute to the literature on how anti-social and unethical behaviour is impacted by environmental stressors such as extreme weather events and air pollution (Bondy et al., 2020; Burkhardt et al., 2019; Goin et al., 2017; Gong et al., 2020; Heilmann et al., 2021; Herrnstadt et al., 2021; Lu et al., 2020, 2018). While much of the previous research has relied on gross proxy measures of social behaviour (e.g. criminal activity), we utilise a set of well-established incentive-compatible economic games to obtain cleaner measures of social behaviour and standard economic preferences. The anti-social behaviour games used in our study are specifically designed to rule out alternative motives for anti-social behaviour and thus allow us to study fundamental aspects of decision-making, independent of contextual factors. In addition, our findings contribute to our understanding of whether the effects of air pollution on economic and behavioural outcomes are of psychological or physiological nature (R. Fehr et al., 2017; Gong et al., 2020). Finally, we explore whether providing air-pollution warnings or alerts can change behaviour, thus contributing to the literature examining the efficacy of providing air quality information (Saberian et al., 2017; Sexton & Timothy, 2016).

Our results indicate that exposure to acute air pollution has no statistically significant effect on anti-social behaviour or economic preferences in the risk and time dimension. Moreover, we find that individuals who received an additional pollution alert do not change their behaviour relative to the control group. However, we do observe that these individuals exhibit significantly less anti-social behaviour compared to the high pollution group which received no alert message (and for which anti-social behaviour slightly increased, on average). With respect to cognition and health outcomes, we find that acute exposure to air pollution significantly decreases self-reported positive affect and increases the likelihood of reporting physical health symptoms. Our findings suggest that air pollution appears to have an acute impact on psychological and physiological well-being, though not enough to induce anti-social behaviour as measured by our experimental games. However, a supplementary analysis provides indicative evidence that anti-social behaviour increases, and altruism decreases for individuals who perceived the pollution episode to be extremely severe.

The chapter is structured as follows: Section 1.2 outlines the research design and describes the experimental procedures, Section 1.3 lays out the estimation strategy, Section 1.4 presents the results and Section 1.5 discusses the implications of our findings.

1.2 Research design

1.2.1 Recruitment and randomisation

Recruitment took place in October 2019 on the campus of Renmin University in Beijing and through online social-media channels, targeting university students from any discipline currently enrolled in either undergraduate or postgraduate degrees at any university located in Beijing. After providing informed consent, participants completed an initial baseline survey, for which they were rewarded 10 Yuan. The primary objective of the baseline survey was to build a subject pool for our experiment and capture basic socio-demographic information and specific baseline preferences. Participants were notified that they would be recontacted later in the semester to complete a second survey. Moreover, to incentivise participation, participants were informed that if they completed both surveys, they would be eligible to participate in a prize draw of 100 Yuan to be awarded to ten students.

In total, 793 participants completed the baseline survey. Of these, 45 respondents were excluded as their university was not located in Beijing, they were currently not in Beijing (e.g., on exchange) or they could not be re-contacted via WeChat by our research assistants. The remaining 748 students were then randomly assigned to either a low pollution control group or one of two high pollution treatment groups using a stratified sample and re-randomisation procedure.

Participants were stratified by gender, university cluster, year of study, Hukou status and perceived air pollution health tolerance across treatments.[2] Within each stratum, every third student was assigned to a given treatment or control group. Randomisation was re-run until balance was achieved for a pre-specified set of control variables deemed important for the study. These included basic demographic and health measures, baseline social preferences following E. Fehr et al. (2002), cooperation (hypothetical investment in a public goods game) and perceptions specific to air pollution in Beijing, the participant's hometown and at their current place of residence. A detailed overview of all variables used for balance checks can be found in Appendix 1.A4.

1.2.2 Manipulating exposure to air pollution and treatments

A persistent challenge to causal identification of pollution effects has been the inability to exogenously manipulate air pollution exposure in a controlled experimental setting. Our study was specifically designed and pre-registered to address this challenge by exploiting naturally occurring variation in air pollution by carefully selecting high-pollution and low-pollution episodes for data collection purposes. Natural variation in air pollution is common in Beijing, especially during the winter-heating period from mid-November to mid-March (Xiao et al., 2015). In Beijing, pollution episodes generally occur over a series of consecutive days with light southerly winds, which transport pollution emitted from industrial compounds into the city. Pollution episodes are often amplified by thermal inversions, which are meteorological phenomena where abnormal temperature profiles in the atmosphere trap air pollution near to the ground (Sager, 2019).[3] Pollution episodes generally come to an abrupt end with the onset of strong winds from north-westerly direction, clearing out the air pollution.

By exploiting this natural discontinuity in air pollution exposure, we are able to survey both treatment and control groups within a timeframe of several days. In order to select appropriate days for data collection, the research team closely monitored local pollution and

[2]Universities were clustered by geographic location within Beijing into North, Central and South. Hukou status refers to whether the participant's household origin is registered as rural or urban in accordance with China's family residence registration system. Perceived air pollution health tolerance was measured on a five-point scale (Not at all to very much) based on the question: "Do you think air pollution will affect your health?".

[3]Thermal inversions are commonly employed as an instrumental variable to identify causal impacts of air pollution. See for example Sager (2019) for a recent application and visualisation of thermal inversion as an instrument for air pollution.

weather forecasts. The primary criterium was the predicted level of air pollution, guided by the categorisation of the official Air Quality Index (AQI). Figure 1.1 shows the official AQI chart divided into six colour-coded categories, where higher AQI values represent higher levels of air pollution and the greater health concern. The AQI is calculated by combining the concentration of five main pollutants including PM2.5, PM10, sulphur dioxide (SO2), nitrogen dioxide (NO2), ozone (O3) and carbon monoxide (CO).

Air Pollution Level	AQI	Description of Air Quality
Good	0 to 50	Air quality is satisfactory, and air pollution poses little or no risk.
Moderate	51 to 100	Air quality is acceptable. However, there may be a risk for some people, particularly those who are unusually sensitive to air pollution.
Unhealthy for Sensitive Groups	101 to 150	Members of sensitive groups may experience health effects. The general public is less likely to be affected.
Unhealthy	151 to 200	Some members of the general public may experience health effects; members of sensitive groups may experience more serious health effects.
Very Unhealthy	201 to 300	Health alert: The risk of health effects is increased for everyone.
Hazardous	301 and higher	Health warning of emergency conditions: everyone is more likely to be affected.

Figure 1.1: Air Quality Index (AQI) categories

In our experiment, participants in the high pollution and high alert treatment groups were invited to complete the survey when air pollution levels were objectively high, with AQI values exceeding 'Very Unhealthy' levels of pollution (AQI > 200). Participants in the control group (low pollution) were invited to complete the survey when air pollution levels are objectively low, with AQI values in the 'Good' range (AQI < 50).

Moreover, as external weather conditions may confound our primary outcomes, a further criterium was to select days with comparable weather conditions. Finally, we hypothesised that students may behave differently mid-week as compared to weekends and Fridays, thus biasing our primary outcomes. We aimed to distribute our experimental surveys only during

Table 1.1: Experimental treatments

	Air Pollution	
Alert	Low	High
No	Low-P (Control Group)	High-P (Treatment 1)
Yes	-	High-P-Alert (Treatment 2)

the first half of the week. While our experimental design allowed us to control for the criteria discussed above, other behavioural factors, in particular behavioural adaptation to air pollution exposure, were beyond our control. We thus included survey questions to capture potential protective behaviour (mask wearing or reducing outdoor time) and whether the participant owned an air purifier.

Finally, all participants were notified 24-hours in advance by direct message to maximise participation. Moreover, this allowed us to introduce an additional experimental manipulation. Participants in the high pollution ‘alert’ group received a slightly longer message informing them about the projected pollution hazard (see Appendix 1.B). This experimental variation was included as air pollution information can be used as an environmental policy tool to increase the salience of air quality and thereby alert people to the risk of being exposed to air pollution (Delmas & Kohli, 2020; Graff Zivin & Neidell, 2009; Saberian et al., 2017). The objective of this treatment was to experimentally assess the efficacy of providing pollution alerts via direct message on days with objectively high levels of air pollution. Specifically, we sought to understand whether alerts significantly influence how pollution is perceived, which in turn could encourage protective behaviours against exposure or directly impact social and economic decision-making. See Table 1.1 for a summary of the experimental treatments.

1.2.3 Experimental timeline and conditions

Data were collected on three days in December 2019. In total, we made use of two pollutions episodes to administer the survey to subjects in the high-pollution treatment groups. The first pollution episode occurred between the 8th – 10th December. The experimental survey was distributed to the High-P and High-P-Alert treatment groups at 5pm on Monday, 9th December. Throughout the survey period, the AQI was above 230, pollution levels considered as very unhealthy. On 10th December, at approximately 12pm, pollution levels dropped with the onset of north-westerly wind and remained low. The experimental survey was then distributed to the Low-P control group at 5pm on Wednesday 11th December. Figure 1.2 shows two pictures taken on the campus of Renmin University at approximately 10am on both the low-pollution day (left) and during the first pollution episode (right).

Figure 1.2: Low pollution day (left) and Pollution Episode 1 (right)

An additional set of respondents from the high-pollution treatment groups were contacted during a second pollution episode on 24th December. Although the pollution forecast predicted AQI to be within the unhealthy (AQI 151-200) to very unhealthy (AQI 201-300) range, the second pollution episode turned out to be less severe than predicted with an average AQI of 105 during the sampling period. The experimental timeline is visualised in Figure 1.3.

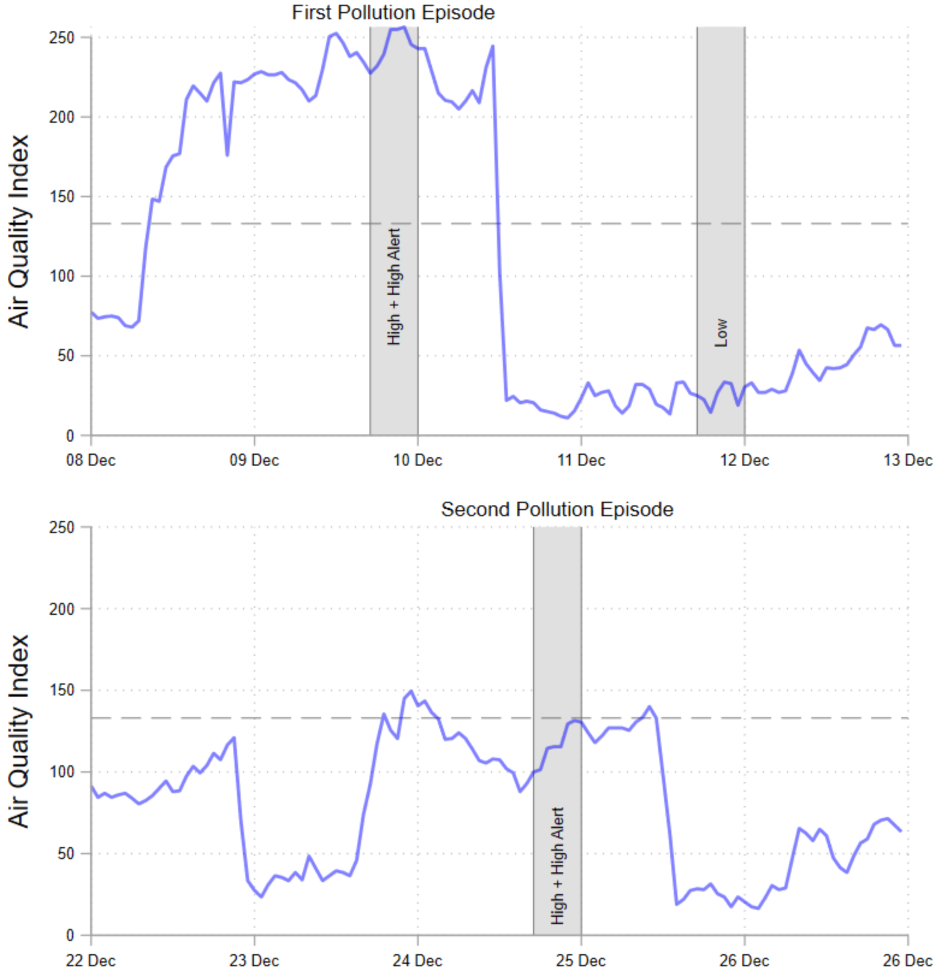

Figure 1.3: Pollution episodes and sampling periods

Note: Dashed line shows annual average $AQI = 133$.

While data collection could have been delayed to a more severe pollution episode in 2020, which are not uncommon in January and February, we were bound by time constraints to complete data collection prior to the start of the exam period (6$^{\text{th}}$ January). Collecting data during the exam period may have confounded our results due to unobservable factors such as exam stress or anxiety. In hindsight, avoiding a further delay to data collection was the correct choice, as all our data was collected prior to the outbreak of Covid-19 in China, which was first officially reported to the WHO on 31$^{\text{st}}$ December.

Furthermore, we were successful in meeting the additional criteria set out in our data collection procedures. All surveys were conducted during the first half of the week, on either a Monday or a Wednesday, and weather conditions were comparable on all three days (-2°C average temperature and approximately 7 sunshine hours, except for during the second pollution episode, which was cloudy throughout).

For all treatment groups, we limited the time-window during which participants could complete the survey from 5pm until midnight. By limiting the response time, we avoid substantial time-of-day effects and ensure that participants were exposed to the same level of air pollution. The survey remained accessible after midnight, and 22 late responses were received. Of these, we included four which were received shortly after midnight, however, those participants who responded after 1am (n = 18) were excluded from the analysis. As expected, nearly half of the participants (46.7%) responded within the first hour. The remaining responses were spread relatively equally across the remaining survey hours (between 5%-15% per hour).

Table 1.2 shows the number of valid survey responses received from each treatment group which were submitted between 5pm and 1am. Moreover, the table displays multiple measures of air pollution during the sampling periods. To obtain accurate measures of air pollution and verify their robustness, we utilise air quality information from multiple sources. First, we computed the mean AQI during each sampling period obtained from the official pollution monitoring station most closely located to the campus of Renmin University (Haidian District). Second, we installed our own pollution monitor on the campus of Renmin University to measure indoor air quality. The monitor was placed inside an office building and produced half-hourly readings for PM2.5 and AQI, which were automatically stored on a hard drive. Our own indoor measurements are very similar to those of the official outdoor monitors, which verifies official measurements and shows that pollution easily penetrates indoors. As previous research has suggested (He et al., 2019; Vette et al., 2001), this observation shows that fine particulate matter can readily penetrate indoor settings and staying indoors on polluted days is unlikely to greatly reduce exposure if no air purifier is installed. Finally, we included a survey question which asked participants to rate today's air quality and serves as a measure of perceived pollution.

Table 1.2: Participants and pollution exposure by treatment group

	N	Pollution Episode	AQI Mean (Official)	AQI Mean (Campus)	Perceived Pollution
Low Pollution	166	0	22.52	11.98	4.54
High Pollution (Ep.1)	149	1	245.32	264.20	7.43
High Pollution + Alert (Ep.1)	163	1	245.26	262.01	7.55
High Pollution (Ep.2)	78	2	105.09	148.13	5.99
High Pollution + Alert (Ep.2)	76	2	105.65	145.24	6.11

Note: Table displays sample size (N), mean AQI values from official and campus air quality monitors, and perceived pollution for each of the treatment groups. Perceived air pollution was measured on an 11-point scale ranging from very low (0) to very high (10) using a single-item measure.

1.2.4 Experimental survey design

Our experimental survey consisted of three experimental modules, a health and well-being questionnaire and a debriefing questionnaire. To encourage truthfulness and effort, all decision-making tasks were incentivised so that participants' choices during the experiment determined their monetary earnings. Moreover, subjects were never deceived. Module I included three well-established experimental games to assess social behaviour: a joy of destruction game; a take game with and without deterrence and a third-party punishment game, which included a dictator game donation decision. In all three decision tasks, respondents' choices had an impact on their own payoff as well as their matched player's payoff, who would be determined after the completion of the survey. Participants were informed that their behaviour would have real financial consequences for their matched player, and payoffs were adjusted corresponding to one randomly selected decision made in Module I.

Module II was designed to elicit standard risk and time preferences and included a lottery choice task (Eckel & Grossman, 2002) and a convex time budget task (Andreoni et al., 2015). Participants made one decision in the lottery choice task and 24 decisions in the CTB task. To increase the stakes, but remain within our budget, participants were informed that 30 respondents would be selected at random and one of their choices from Module II would be selected at random to determine the pay-off for that part of the survey.

In Module III, we systematically assessed participant's cognitive functioning and psychological well-being. Participants first completed a set of Raven's Matrices (or puzzles) and a Numerical Stroop Task to assess cognitive ability and self-control. In both tasks, correct answers were financially rewarded to incentivise effort. The remaining questions in Module III were non-incentivised self-report measures of psychological well-being. We utilised clinically verified multi-item scales which are well-known in the psychological literature. Table 1.A1 in the Appendix provides a detailed overview of the survey modules, experimental tasks and corresponding outcome variables. All instructions were provided in Chinese, and all choices that involved monetary payoffs were framed in terms of Chinese Yuan (CNY). The English translation of the instructions is included in the Online Appendix 1.C.

Every participant received a "show up fee" of 10 Yuan (= £1.10) for completing the survey. Their final bonus payment was determined by one randomly selected decision from Part I, one randomly selected decision from Part II (if they were one of the thirty selected participants) and the number of correct choices made in the cognitive ability tasks. Moreover, once all data collection was completed, ten participants were selected at random to receive a bonus of 100 Yuan (£11.13) for completing both the baseline and the experimental survey. Once the final payoff had been calculated, participants received the money via WeChat's built-in money transfer tool (WeChat Transfer) on the following day. Time preferences payments from Part II were delivered according to the time schedule indicated in the selected decision task (today or in five or in nine weeks).

1.2.5 Experimental outcomes

In the following, we describe in more detail the experimental tasks and the outcome variables constructed for our analysis. As per our pre-registration, section 1.2.5 describes the primary outcomes and Sections 1.2.5 and 1.2.5 describe the secondary outcome variables.

Social behaviour

The Social Behaviour Module consisted of three separate incentivized games to elicit different dimensions of people' willingness to engage in anti-social behaviour. The Joy of Destruction Mini-game (JOD) game provides a measure of nasty behaviour (Abbink & Herrmann, 2011). In this two-player game, participants are anonymously matched in pairs and then face the binary decision whether to destroy their assigned partner's endowment by half at a cost to themselves, or maintain the status quo. In our setting, both players are endowed with 20 Yuan (£2.23) and can decide to destroy 10 Yuan of the other player's endowment, at an own cost of 2 Yuan. Participants are further informed that there is a one third probability that the player loses 10 Yuan anyway, regardless of the other player's decision, rendering the other player's decision to destroy the endowment ineffective (Abbink & Herrmann, 2011). The design of JOD game removes all conventional motivations for anti-social behaviour and further allows destructive behaviour to be partially hidden behind a component of random destruction. The main outcome variable obtained from this task is the binary decision of whether to destroy half of the other player's endowment.

The Take Game provides a measure of covert anti-social behaviour in the form of stealing or theft (Schildberg-Hörisch & Strassmair, 2012). In this two-player game, participants are anonymously matched and provided unequal endowment of 10 Yuan or 18 Yuan. Participants then decide whether to take from the other player's initial endowment in two different scenarios. In the first scenario, players can take any amount without facing any consequences. In the second scenario, they can take any amount, but face a 60% probability of being detected, effectively reducing their payoff to 6 Yuan due to a penalty. In both scenarios the decisions are not observed by the other player. Two outcome variables are obtained from this task: the amount taken (in Yuan) from the other player's endowment with and without the risk of being detected (i.e., with deterrence).

The Third-Party Punishment Game provides a measure of pro-social behaviour in a setting where the transfer amount is observed by a third party, as well as a measure of third-party sanctions for violations of a distribution norm (E. Fehr & Fischbacher, 2004). As in a classic dictator game, players first decide whether to transfer part of their 20 Yuan endowment to an anonymously matched recipient. Dictators could transfer up to 10 Yuan, in steps of 2, but could also decide to transfer nothing. They then took on the role of a third party observing another player's transfer decision, with the option to enact costly punishment for each possible transfer amount sent by the observed dictator. In our setting, the third-party observer had an endowment of 10 Yuan and could use any of this amount to punish the dictator by reducing their endowment by a factor of three (e.g., 2 Yuan would reduce the dictator's endowment by 6 Yuan). Players were informed of the third role, the passive recipient of the amount transferred by the dictator, who made no decision. As the game involved multiple roles and players faced several decisions, participants were informed that they would be randomly assigned to one of the roles and that one of their choices would be randomly selected to determine their own and the other player's payoffs. From this game we obtained two primary outcomes: an incentivised measure of giving from the dictator game, as a measure of pro-social behaviour under observability, and the amount punished if the assigned dictator transfers zero.[4]

[4]For our analysis, we selected to explore sanctioning behaviour for the most unequal distribution (i.e., dictators giving nothing to the recipient). We also observed punishment decisions for each of the alternative

Economic preferences

The Economic Preferences Module consisted of two tasks and a total of 24 decisions. To increase incentive-compatibility, we significantly increased the potential payoffs for this Module to range between range 56 – 140 Yuan (£6 - £16) based on participants' decisions. Moreover, participants were informed that 30 students would be chosen at random to receive a bonus payment from this Module, which would be determined by one of their decisions.

Risk preferences were obtained using a standard incentivised Lottery Choice Task (Eckel & Grossman, 2002). In this task, participants decide between six lotteries, each with a 50% chance of paying a lower or higher amount. Lotteries are increasing in variance, total pay-off and riskiness (see Table 1.A2). From the lottery choice task, we obtain the participant's constant relative risk aversion (CRRA) parameter interval, with higher CRRA parameters indicating greater risk aversion.[5] Following Ong et al. (2019), we replace the infinity value for the lower bound with -1 and for the upper bound with 10, which allows us to calculate the CRRA midpoint between lower and upper bound for each individual. The CRRA midpoint serves as our primary measure of risk aversion for the main analysis.[6]

Time preferences were elicited using Convex Time Budgets (CTB) following Andreoni et al. (2015). Participants made 24 consecutive decisions between sooner or later payments, across four different timeframes, with six budget lines for each timeframe. Participants thus faced decisions over payment "today and 5 weeks from today", "today and 9 weeks from today", "5 weeks from today and 10 weeks from today" and "5 weeks from today and 14 weeks from today", replicating the design in Andreoni et al. (2015) and using scaled payoff values as in Y. Chen et al. (2019) to maintain the interest rates associated with later payment of the original experiment (Andreoni et al., 2015). The 24 budget lines are displayed in Appendix Table 1.A3. For our analysis, we estimate the individual-level parameters beta and delta parameters capturing present biasedness and discounting, respectively.

transfer amounts (2, 4, 6, 8 and 10 Yuan). For robustness, we additionally explore sanctioning behaviour to enforce a 50/50 distribution norm, based on the amount subjects were willing to punish if the dictator transferred half of their endowment (i.e., 10 Yuan).

[5]The CRRA parameter intervals are: (-3.46, $+\infty$) for those who chose (56/56); (1.16, 3.46) for (48/72); (0.71, 1.16) for (40/88); (0.50, 0.71) for (32/104); (0, 0.50) for (24/120); and ($-\infty$, 0) for (4/140).

[6]An alternative and more common approach is to utilise both lower and upper CRRA parameters as dependent variables in an interval regression. However, using the CRRA midpoint allows us to standardise the outcome for risk aversion, which facilitates visualisation and comparability. We run additional interval regressions for robustness purposes.

Cognition and health

To measure cognitive functioning, we used a subset of Raven's Standard Progressive Matrices (Bilker et al., 2012) and paid subjects 0.5 Yuan for each correct answer. Cognitive performance was indexed by the sum of correctly solved matrices (range 0-9). After the Raven's Matrices, participants completed a Numerical Stroop Task as in Mani et al. (2013). The task consisted of 24 three-second trials in which participants were shown a multi-digit number (e.g., 888) and had to identify and input the number of times the digit is repeated, (i.e., 3, in this example) rather than the digit itself. Each correct answer was rewarded with 0.3 yuan. In contrast to our expectations, the vast majority of participants completed nearly all trials correctly (mean = 23.1), providing insufficient variation to utilise the correct completions score as an outcome variable for our analysis.

In addition to cognitive ability, we also measured participants' momentary state of ego-depletion, which reflects an individual's self-control capacity at a given moment, according to ego-depletion theory (Baumeister et al., 1998).[7] This measure was obtained using a modified 5-item Depletion Scale adapted from Twenge et al. (2004) where a higher score indicates higher levels of depletion. Participants were asked to evaluate how they felt at the current moment based on the following five items: "I feel drained", "I feel calm and rational", "I feel lazy", "I feel sharp and focused" and "I feel like my willpower is gone". Responses were given on a 5-point Likert scale ranging from 1 "not true" to 5 "very true".

The well-being module consisted of a selection of survey questions to capture different dimensions of well-being spanning a range of time frames. Our goal was to capture subjective well-being, ranging from momentary emotions and affect to global evaluations of life satisfaction. To measure short-term mood on the day of the survey, we use the international short form of the Positive and Negative affect Schedule (PANAS-ISF) consisting of a 10-item self-reported questionnaire (Thompson, 2007). Using the respective negative and positive affect items (five each), we construct scores for positive and negative affect, where higher scores indicate greater presence of positive or negative mood on the day of the survey. We used the Center for Epidemiologic Studies Depression Scale (CESD) 10-item scale as a validated self-reported instrument to measure the prevalence of depressive symptoms (Andresen et al., 1994). Participants were asked to rate each item (e.g., I find it difficult to do anything) based on the frequency that each mood or symptom occurred during the past week on a four-point scale, ranging from zero ("none of the time") to three ("most of the time"). We construct a

[7] While the theory of ego depletion is very prevalent in the psychology literature, more recently, it has also attracted attention in economics and there is a growing number of studies which have assessed the impact of self-control depletion on economic preferences (Achtziger et al., 2018, 2016; Gerhardt et al., 2017).

depression score (range 0-30) by totalling all item scores, where a higher score represents greater depression (Andresen et al., 1994). Following, Andresen et al. (1994) we construct a binary indicator classifying subjects as having “depressive symptoms” for those who score equal to or above a cut-off score of 10. The binary indicator is employed in our main analysis.

In addition to mental health, we asked participants to self-report their physical and general health. Physical health was assessed based on binary reports of whether a participant had experienced a range of physical symptoms during the past week (see Appendix Section C4 Q8). For our analysis, we created an index of physical health based on three symptoms commonly associated with pollution exposure (cough, sore throat, stuffy nose). The index was constructed by taking the average of the z-scored (standardised) variables, following Kling et al. (2007). General health status was assessed using responses to a question asking participants to rate their general health condition on a scale of 1 to 5, indicating very poor to very good health status on the day of the survey. Finally, we asked participants to self-report their sleep quality in general and specific to the night before the survey day. We use the latter in our analysis.

1.2.6 Summary statistics

Table 1.3 presents summary statistics for all outcome variables employed in the analysis, as well as the socio-demographic characteristics of our analysis sample. The section on ‘Social & Economic Preferences’ comprises our primary outcomes on social behaviour and secondary outcomes for risk and time preferences. With respect to anti-social behaviour, 16% of participants chose to destroy their matched player’s payoff, which is slightly lower than the destruction rate (25.8%) of the original experiment conducted with students in Ukraine (Abbink & Herrmann, 2011). Participants took, on average, 10.12 Yuan from their counterpart and only slightly less (9.56 Yuan) if there was a risk of being detected. The average amount transferred to an anonymous partner in a dictator game decision (with observability) was 3.94 Yuan. Participants chose to spend on average 2.12 Yuan to enact punishment (multiplied by a factor of three) if the dictator transferred zero in the preceding dictator game decision, thus showing a significant willingness to enact costly punishment to enforce a pro-social norm. With respect to time preferences, the sample mean of the individual-level beta and delta parameters are 0.92 and 0.98, respectively, which are in line with previous estimates (Imai et al., 2021) and indicate a slight degree of present bias in our sample population.

The section on ‘Cognition & Health’ presents additional secondary outcomes in the health and cognition domain including (i) cognitive ability and depletion, (ii) emotional affect and depressive symptoms and (iii) health variables (general health, sleep quality). The majority of health outcomes were measured as scores constructed from multi-item screening questionnaires or single-item Likert scale survey instruments, and thus must be viewed within their respective response range (Min and Max). A noteworthy observation is that 67% of participants scored above the 10-point threshold on the CESD Scale, thus indicating the presence of depressive symptoms.

The section on ‘Socio-demographic Characteristics’ summarises a selection of indicators obtained from the baseline survey conducted in October 2019. We find that our final sample consists primarily of younger undergraduate students (mean age of 20 and mean year of study 2.5), was primarily from an urban household background (Hukou) and the majority of participants were female (78%). Most students were not local to Beijing but had spent on average 6.24 years living in the city at the time of the baseline survey. Finally, we explored whether participants believed that air pollution had an impact on their health, with the majority stating that air pollution had a stronger impact (mean = 3.89).

1.2.7 Balance

Our initial stratified randomisation procedure was successful in achieving balance across all five groups, however, we observe slightly higher non-response rates in each of the treatment conditions (see Table 1.2). We therefore run a series of balance checks to ensure that treatment effect estimates are not confounded by differences in individual-level characteristics. We first explore the set of variables which were utilised for the initial balance checks during the randomisation procedure (see Appendix Table 1.A4). Overall, we observe that balance was maintained across all three groups, which differ only slightly with respect to baseline health status. Here we observe that self-reported health status is slightly higher in the high-alert group compared to both the control group and the high pollution treatment group, significant at the 10% and 5% level, respectively.

While balance is maintained with respect to socio-demographic characteristics and general perceptions of air pollution, we may be concerned that differential non-response is in some form related to social or economic preferences, which may bias our main results. To address this concern, we draw on our rich baseline dataset to check the balance of baseline social preferences, an incentivised measure of risk aversion, willingness to compete and mental health, all of which were measured during the baseline survey in October 2019.[8] Appendix

[8]The baseline survey was collected over a series of days in October during which the air pollution levels were consistently low. We thus argue that preferences measured at baseline are independent of pollution exposure.

Table 1.3: Summary statistics

	Mean	SD	Min	Max	N
Social & Economic Preferences					
Joy of Destruction (Destroy = 1)	0.16	0.36	0.00	1.00	632
Taking (¥)	10.12	6.30	0.00	18.00	632
Taking with Detterence (¥)	9.56	6.83	0.00	18.00	632
Dictator Giving (¥)	3.94	3.45	0.00	10.00	632
Punish (Punish = 1)	0.59	0.49	0.00	1.00	632
Punishment (¥)	2.12	2.39	0.00	10.00	632
Risk Aversion (CRRA midpoint)	2.96	2.83	-0.50	6.73	632
Present Bias (β parameter)	0.92	0.20	0.00	1.00	622
Patience (δ parameter)	0.98	0.09	0.00	1.00	622
Cognition and Health					
Cognitive Ability (correct puzzles)	6.48	1.49	1.00	9.00	632
Depletion (score)	0.43	0.73	-1.40	2.40	632
Depressive Symptomns (Yes = 1)	0.67	0.47	0.00	1.00	632
Negative Affect (score)	10.36	4.27	5.00	25.00	632
Positive Affect (score)	13.21	3.41	5.00	24.00	632
Physical Health (index)	0.00	0.68	-0.37	2.73	632
General Health (scale)	3.70	0.83	1.00	5.00	632
Sleep Quality Last Night (scale)	7.48	1.81	1.00	10.00	632
Socio-demographic Characteristics					
Age	19.90	1.54	17.00	29.00	632
Female	0.78	0.42	0.00	1.00	632
Only Child	0.65	0.48	0.00	1.00	632
Rural Hukou	0.20	0.40	0.00	1.00	632
Economics/Finance Major	0.45	0.50	0.00	1.00	632
Year of Study	2.58	1.16	1.00	6.00	632
Airpollution impacts my health	3.89	0.92	1.00	5.00	632
Years living in Beijing	6.24	6.88	1.00	22.50	632

Note: Table displays the summary statistics for the analysis sample (N = 632). *Score* refers to variables constructed from multi-item survey measures; *scale* refers to variables constructed from single-item survey measures; *index* refers to variables combining (averaging) multiple standardised single-item variables.

Table 1.A5 shows the sample means for low, high and high-alert groups and the differences between means accompanied by significance stars depicting the level of statistical significance obtained from a two-sided t-test. In addition to the previously discussed difference in health status, we observe only one additional statistically significant difference in means. The High Alert group invested on average slightly more (range $0 - 100$ Yuan) in the hypothetical Trust Game (Berg et al., 1995), which mimics an investment decision with uncertain returns, compared to the control group. We thus conclude that randomisation was successful in achieving balance across all three groups, despite differential non-response rates.

1.3 Estimation

The main specification estimates the treatment effect of air pollution on the outcome of interest as follows:

$$Y_i = \beta_0 + \beta_1 High_i + \beta_2 High_i \times Alert_i + \beta_3 EP2_i + \beta_4 H_i + S_i' + \varepsilon_i \tag{1.1}$$

where i references individual and Y_i is the outcome of interest. $High_i$ is the treatment indicator equal to one for individuals in the High Pollution treatment group; $Alert_i$ is an indicator for individuals that received an additional pollution alert message. The coefficient β_2 on the interaction term $High \times Alert_i$ thus identifies the difference between individuals in the high-pollution group that received a pollution alert and those that did not. The coefficient β_1 is the estimated difference between the low-pollution group and the high-pollution group (that did not receive an alert message). $EP2_i$ is an indicator identifying individuals that were surveyed during the less severe second pollution episode; H_i controls for baseline general health status, the only socio-demographic variable that is unbalanced across groups (see section 1.2.7). Since the randomisation took place after stratifying on gender, university cluster, year of study, Hukou status and perceived air pollution health tolerance, we additionally control for these variables (S_i') to increases statistical precision, as recommended by Bruhn and Mckenzie (2009). Heteroscedasticity robust (Eicker-Huber-White) standard errors are computed and displayed throughout the analysis.

Prior to the main analysis of time-preference outcomes, parameter estimates for present bias (beta parameter) and discounting (delta parameter) must first be structurally estimated at either the aggregate level (for each treatment group) or individual level (for each participant). For completeness, we estimate both the aggregate and individual-level parameters via non-linear least squares estimation, following Andreoni et al. (2015). In our main analysis, we utilise individual parameter estimates, which were bottom and top coded to fall within a range of 0 and 1, before standardization (following the approach in Almås et al. (2019)). This approach facilitates the comparison between outcome variables and visualisation of the main results. Individual-parameter estimates could not be estimated for 10 individuals (1.6% of the sample) who always made the same decision in each of the 24 budget lines, due to insufficient variation. For robustness, we additionally estimate the aggregate beta and delta estimates and their corresponding standard errors for each treatment condition. Differences between aggregate beta and delta estimates are compared using a standard t-test without additional control variables. The results of this analysis are presented in the Appendix.

It is important to note that throughout our analysis, we estimate intention-to-treat (ITT) effects, exploiting random assignment to low and high-pollution groups as a source of exogenous variation in exposure to air pollution. While we limit our sample to respondents who were physically located in Beijing and completed the survey within a pre-specified time frame, our estimates are likely biased towards zero due to imperfect exposure to air pollution. For instance, individuals may take additional protective behaviours to avoid exposure to air pollution. Note that we do not control for protective behaviours on the day of the survey (e.g. mask-wearing), as these may themselves be a consequence of air pollution and thus may raise concerns about "bad control" bias (Angrist & Pischke, 2008). Nonetheless, the downward bias is likely to be small, as students typically stay on campus during weekdays and, as previously shown, are not greatly protected by staying indoors (T. Y. Chang et al., 2019; He et al., 2019).

1.4 Results

Here we present the treatment effect of acute air pollution exposure on our primary and secondary outcomes of social behaviour, economic preferences and cognition and health. First, we investigate whether our pre-specified study design and sampling procedure was successful in manipulating the air pollution levels that subjects were exposed to while completing the experimental survey.

1.4.1 Manipulation checks

Objective air pollution

As discussed above, Figure 1.3 and Table 1.2 provided a clear indication that individuals surveyed during both pollution episodes were exposed to substantially higher levels of air pollution in both indoor and outdoor settings, compared to the control group. For completeness, Figure 1.4 visualises the mean AQI (and corresponding 95% confidence intervals) for each of the treatment groups. All differences in means between the pollution episodes and the low pollution group are highly statistically significant, as indicated by the 95% confidence intervals.

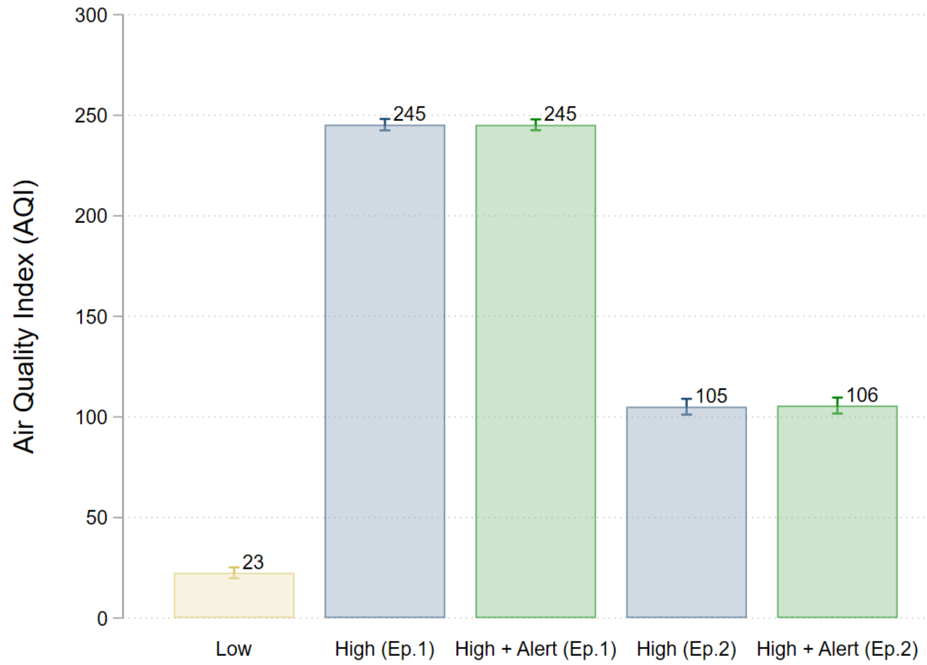

Figure 1.4: Mean Air Quality Index (AQI) from official sources by treatment group

Note: Higher AQI = More Pollution. Error bars indicate 95% confidence intervals.

Subjective air pollution

Next, we conduct a series of manipulation checks to assess whether treatment assignment into high and high alert groups, based on objective levels of air pollution, is reflected by perceived air pollution on the days the survey was administered. To measure perceived pollution, we included a set of questions at the end of the experimental survey asking participants to rate today's air pollution on a scale of zero to ten across four dimensions (general, visual, smell and media). Figure 1.5 displays the mean (and corresponding 95% confidence intervals) of the general perception of pollution on the day of the survey for each of the treatment groups. A visual assessment suggests that there were significant differences in perceived air pollution across the three days on which the surveys were administered. Moreover, comparing objective

(Figure 1.4) and perceived pollution (Figure 1.5) suggests that participants have a relatively accurate perception of air pollution.

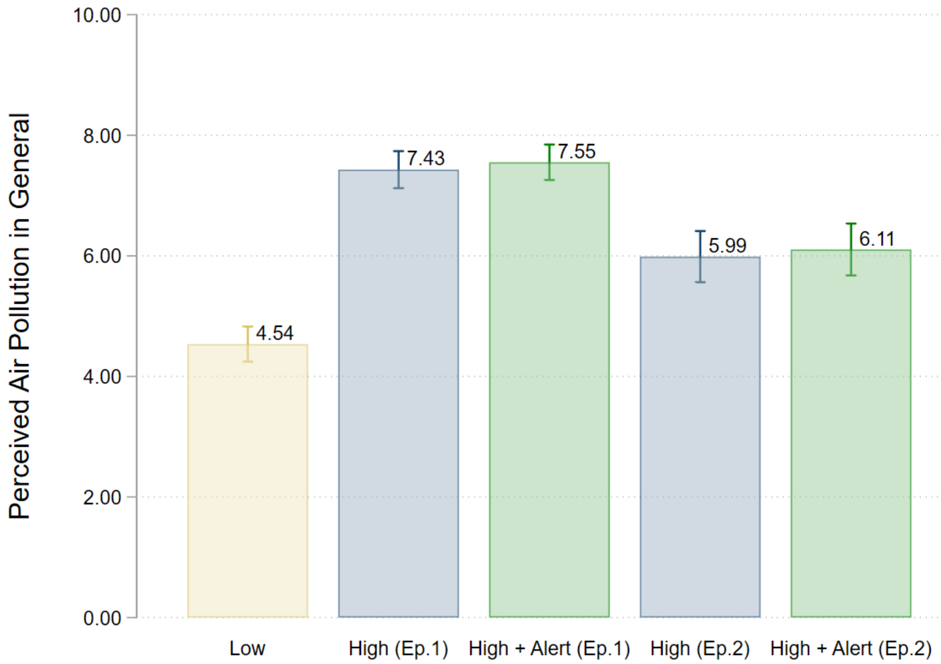

Figure 1.5: Perceived air pollution on the day of the survey by treatment group

Note: Perceived air pollution was measured on an 11-point scale ranging from very low (0) to very high (10) using a single-item measure. Error bars indicate 95% confidence intervals.

To confirm our visual assessment, we regress each of the perceived pollution measures (general, visual, smell and media coverage) on indicators for high and high-alert treatment groups for both pollution episodes separately (low-pollution control group is the omitted category). Moreover, we control for individual characteristics measured at baseline, which may influence the way in which air pollution is perceived. Results are presented in Appendix Table 1.A6 and suggest that pollution was perceived to be significantly higher (in general, via smell and visual assessment) in all four groups, compared to the control group, surveyed on a low pollution day. Participants of the first pollution episode reported significantly higher levels of air pollution across all four dimensions compared to the low-pollution control group. Participants surveyed during the second pollution episode also report significantly higher average perceived pollution, relative to the low-pollution group. However, media coverage of air pollution is not perceived to be statistically different to the low-pollution day.

In sum, the results from this analysis suggest that differences in pollution between the low-pollution day and the two pollution episodes were substantial, based on objective measurements and subjective perception. We thus believe, that our research design was successful in exposing individuals to varying degrees of air pollution, as pre-specified in our pre-analysis plan. While pollution exposure was clearly more extreme during the first pollution episode, the second pollution episode was nonetheless perceived as significantly more polluted than the low-pollution survey day. We therefore utilise all available data by pooling pollution episodes one and two for our main analysis.[9]

Awareness of air pollution ('alert')

In addition to manipulating air pollution exposure, half of the participants surveyed during the pollution episodes were sent a pollution warning as part of the survey reminder message sent to all participants 24-hours prior to the survey launch (see Appendix 1.B).

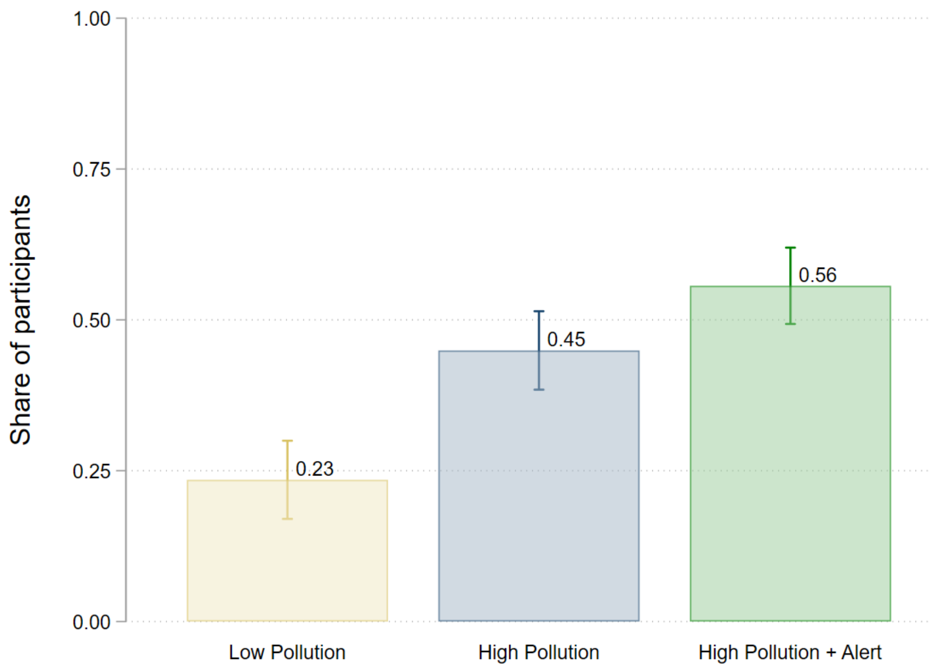

Figure 1.6: Received and read pollution alert message

[9] Pooling the data does not significantly alter our results, compared to using only the data from the more 'extreme' first pollution episode and thereby increases statistical power.

Figure 1.6 shows the share of participants (and corresponding 95% confidence intervals) which stated that they had received and read a pollution alert on the day of the survey, pooling responses from both pollution episodes. This share is the highest in the High Pollution Alert group, significantly different from both the Low and High pollution groups, significant at the 1% and 5% level, respectively. The differences suggest that our messaging strategy was successful in alerting participants about air pollution.

1.4.2 Social behaviour and economic preferences

This section investigates whether social behaviour and economic decision-making differs between the high-pollution and the low-pollution groups, and whether subjects in the high pollution groups make different choices if they received an additional pollution alert message. All the tasks used to elicit social and economic decision-making were fully incentivised to ensure incentive-compatible behaviour.

Table 1.4: Anti-social behaviour

	(1) Destruction	(2) Taking	(3) Taking (Det.)
High Pollution	0.034	0.695	0.020
	(0.040)	(0.674)	(0.744)
High Pollution × Alert	-0.026	-1.364**	-0.840
	(0.034)	(0.587)	(0.640)
Constant	-0.030	10.221***	10.881***
	(0.120)	(1.898)	(2.139)
R^2	0.013	0.019	0.011
Observations	632	632	632

Note: OLS estimates of equation (1.1). In the first column, the dependent variable is an indicator variable for whether the subject chose to destroy their counterpart's endowment (or not) in the Joy of Destruction Game. In the second and third columns, the dependent variable is a continuous measure of the amount taken (in ¥) from their counterpart in the Take Game, without and with deterrence incentives (i.e. a risk of being detected). High Pollution is an indicator identifying individuals randomly assigned to complete the survey during a pollution episode. Alert is an indicator identifying individuals that received an additional pollution alert message 24-hours prior to completing the survey. Robust standard errors in brackets.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

First, we explore whether anti-social behaviour is affected by pollution exposure. Table 1.4 presents the OLS estimates from equation (1.1) for the Joy of Destruction Game (Abbink & Herrmann, 2011) and the Take Game with and without deterrence incentives (Schildberg-Hörisch & Strassmair, 2012). In column (1) we observe that subjects in the high-pollution group were on average 3.4 percentage points more likely to destroy their counterpart's endowment in the Joy of Destruction Game. In the Take Game, the high-pollution group took on average 0.7 Yuan more from their counterpart than the low-pollution group (Column 2), which took on average 10.22 Yuan, after controlling for baseline health status and stratification variables (as specified in equation 1). While these first two results indicate a slight increase in anti-social behaviour in the high-pollution group, the differences are small and not statistically significant. In the variation of the Take Game where there is a 60% chance of being caught

(Column 3), taking behaviour is also indistinguishable between the low and high pollution groups.

The 'High Pollution × Alert' estimates in Table 1.4 show the differences in anti-social behaviour between the high-pollution group and the high-pollution alert group. Here, we observe that receiving an alert message had an attenuating effect on anti-social behaviour. Subjects that received our alert message took significantly less (1.36 Yuan) than those that did not receive a pollution warning in the Take Game (Column 2). This difference is statistically significant at the 5% level. Similarly, average destructive behaviour and taking was lower in the high-pollution alert group in both the Joy of Destruction Game and the Take Game with deterrence, however differences are not statistically distinguishable from zero.

Next, we explore further dimensions of social behaviour with measures obtained from a third-party punishment game, which combines a dictator game transfer decision with a third-party sanctioning decision (E. Fehr & Fischbacher, 2004). The corresponding OLS estimates of equation (1.1) are presented in Table 1.5. In the transfer decision, the high-pollution group gave slightly more (0.27 Yuan) on average than the low-pollution group. Moreover, subjects in the high-pollution group were 1.7 percentage points more likely to punish dictators that gave zero (Column 2), and they were willing to spend slightly more (0.28 Yuan) to punish dictators that gave nothing (Column 3). None of these differences are statistically significant at the 10% level.

Table 1.5: Norm-enforcement

	(1) Giving	(2) Punish (%)	(3) Punishment (¥)
High Pollution	0.268	0.017	0.280
	(0.371)	(0.053)	(0.255)
High Pollution × Alert	0.200	0.041	-0.004
	(0.324)	(0.046)	(0.228)
Constant	4.728***	0.384**	1.109
	(1.047)	(0.153)	(0.711)
R^2	0.018	0.022	0.016
Observations	632	632	632

Note: OLS estimates of equation (1.1). All three outcomes are obtained from the Third Party Punishment Game. In the first column, the dependent variable is a continuous measure of the amount given (¥) in a Dictator Game transfer decision. In the second column, the dependent variable is an indicator for whether the subject (in the role of a third party) chose to punish a dictator that gave zero. In column three, the dependent variable is a continuous measure of the expenditure (i.e. willingness to pay in ¥) to punish a dictator that gave zero (by a factor of three). High Pollution is an indicator identifying individuals randomly assigned to complete the survey during a pollution episode. Alert is an indicator identifying individuals that received an additional pollution alert message 24-hours prior to completing the survey. Robust standard errors in brackets.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Subjects in the high-pollution alert group behaved slightly more pro-socially, giving 0.20 Yuan more than those who did not receive an alert message on the high-pollution day, but were also more likely to punish (4.1 percentage points). The expenditure in punishment did not differ between the two groups. Again, all differences were not statistically different from zero, suggesting that the alert message had no effect on giving and norm-enforcement on the high-pollution day.

Table 1.6 presents the results obtained from estimating equation (1.1) with our secondary outcome measures of risk aversion from a lottery choice task (Eckel & Grossman, 2002) and time-preferences from a Convex Time Budget task (Andreoni et al., 2015). Column (1) presents differences in the CRRA interval midpoints and the dependent variables in columns (2) and (3) are the individual-level parameter estimates for beta (present bias) and delta (patience), estimated via non-linear least squares following Andreoni et al. (2015). The literature exploring the link between air pollution and crime has suggested changes in risk aversion and intertemporal decision-making may be potential mechanisms through which air pollution increases crime, the most plausible channel being via increased discounting (Bondy et al., 2020). While we find no evidence that air pollution exposure affects anti-social behaviour (see Table 1.4), we explore these potential pathways nonetheless, by estimating the direct effect of pollution exposure on risk aversion, present bias and patience.

Table 1.6: Risk and time preferences

	(1) Risk Aversion	(2) Present Bias	(3) Patience
High Pollution	-0.045	-0.009	-0.000
	(0.308)	(0.020)	(0.011)
High Pollution × Alert	-0.281	0.013	0.011
	(0.262)	(0.019)	(0.008)
Constant	1.533*	-0.879***	0.989***
	(0.878)	(0.052)	(0.021)
R^2	0.015	0.009	0.013
Observations	632	622	622

Note: OLS estimates of equation (1.1). In the first column, the dependent variable is a measure of risk aversion given by the CRRA parameter interval midpoint, obtained from a Lottery Choice Task (Eckel & Grossman, 2002). In the second and third columns, the dependent variables are the individual-level beta (present bias) and delta (patience) parameter estimates derived from a Convex Time Budgets task, following Andreoni et al. (2015). Individual-level estimates were estimated via non-linear least squares and bottom and top coded to fall within a range of 0 and 1. The coefficients in column (2) are inverted so that a positive treatment effect indicates greater present bias

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

As Table 6 shows, the high pollution and low pollution group made similar risk choices in the lottery choice task. The high-pollution group behaved slightly less risk averse, however, the difference is hardly distinguishable from zero and not statistically significant. Moreover, we find no differences in present bias (Column 2) or patience (Column 3), suggesting that intertemporal preferences are not affected by pollution exposure in our setting.

For risk aversion, we find comparable null effects when using the CRRA interval (upper and lower bounds) as dependent variables in an interval regression (see Appendix Table 1.A7). For present bias and patience, our analysis of aggregate beta and delta parameter estimates suggests that there are no statistically significant differences between treatment conditions (see Appendix Table 1.A7).

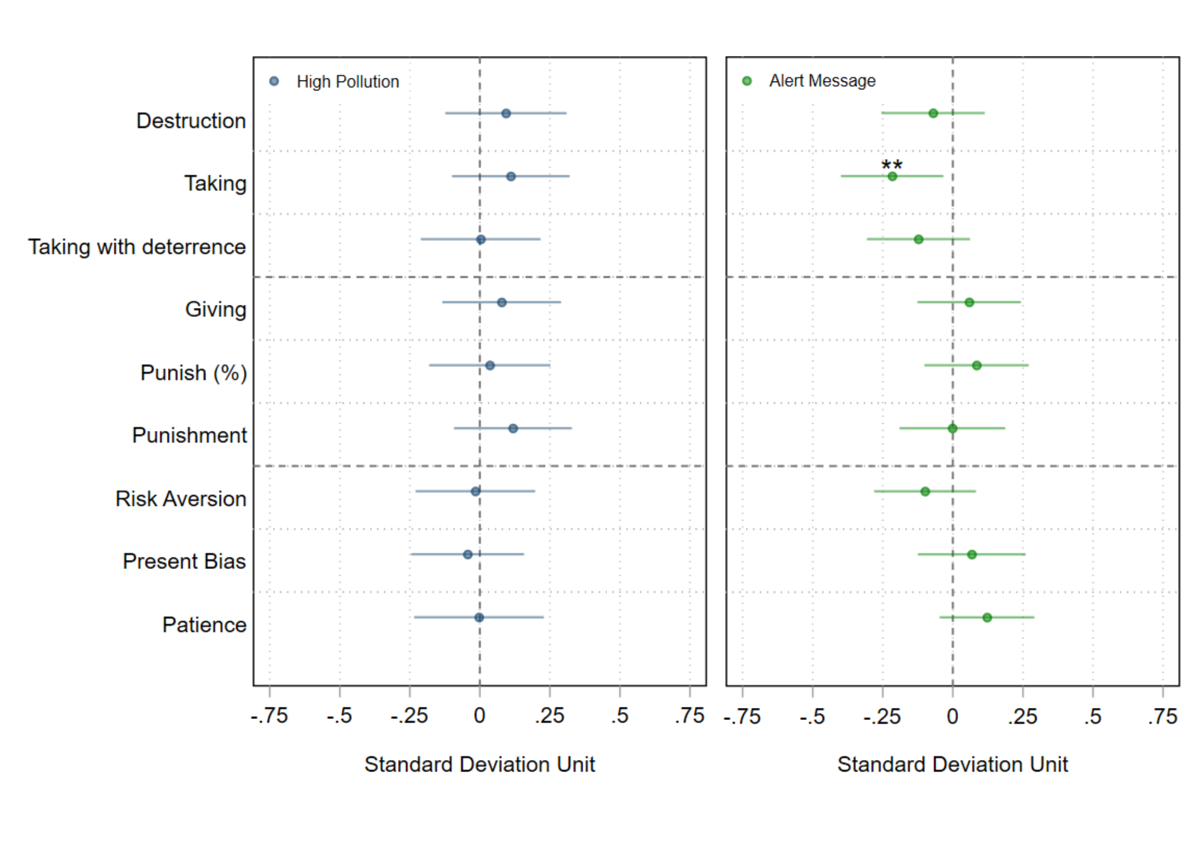

Figure 1.7: Results summary: Social behaviour and economic preferences

Note: The left panel displays the difference between the 'high-pollution' treatment group and the 'low-pollution' control group. The right panel shows the difference between 'high-pollution group' and 'high-pollution alert' group. All dependent variables were standardized (z-scored) on the mean prior to analysis. Estimates for Present Bias and Patience are the individual-level beta and delta parameters estimated via non-linear least squares following Andreoni et al. (2015). Parameter estimates were bottom and top coded to fall within a range of 0 and 1, prior to standardisation. Moreover, the treatment effect for present bias was flipped so that a positive treatment effect indicates greater present bias. Error bars indicate 95% confidence intervals.

Figure 1.7 summarises the main results presented in this section. The left panel visualises the standardised treatment effect of acute exposure to high pollution levels whilst completing the survey. The right panel visualises the difference between the high pollution and the high-pollution alert groups, thus capturing the effect of receiving a pollution warning prior to completing the survey on a polluted day. As previously discussed, we utilise all available data by pooling both pollution episodes. Estimates are obtained from equation (1.1) using standardised dependent variables to allow for a direct comparison of treatment effects in units of standard deviations across different outcomes.

While our study design was successful in exposing individuals to varying degrees of air pollution (see Section 1.4.1), we find that social behaviour and economic preferences are unaffected by acute pollution exposure. We find some indication that being exposed to high pollution slightly increases anti-social behaviour in the form of increased destructive behaviour (0.09 SD) and taking (0.11 SD), however, the differences are not statistically distinguishable from zero. Moreover, providing an additional pollution alert appears to slightly attenuate the effect of high-pollution exposure on anti-social behaviour and leads to a statistically significant reduction in taking behaviour (0.22 SD), when there is no risk of being detected.

With respect to risk and time preferences, we observe that all estimates are close to zero and statistically insignificant, suggesting that air pollution has no effect on standard economic preferences in the risk and time dimension for individuals in our sample.

1.4.3 Cognition and health

In this section we present results for our cognition and health outcomes (pre-registered as secondary outcomes). As in the previous section, we compare the high pollution to the low pollution group and additionally explore whether receiving a pollution alert had an impact on cognitive performance and self-reported health measures. We measure cognitive ability using an incentivised task and primarily rely on clinically verified multi-item screening questionnaires to assess participants health status. Both changes in cognition and health have often been thought to explain the relationship between air pollution and economic decision-making (Chew et al., 2021).

Table 1.7: Cognition

	(1) Cognitive Ability	(2) Momentary Depletion
High Pollution	-0.054	0.013
	(0.157)	(0.076)
High Pollution × Alert	-0.116	0.015
	(0.140)	(0.065)
Constant	6.377***	1.150***
	(0.471)	(0.243)
R^2	0.027	0.076
Observations	632	632

Note: OLS estimates of equation (1.1). In the first column, the dependent variable is a measure of cognitive ability given by the number of correctly solved Raven's Matrices (Bilker et al., 2012). In the second column, the dependent variable is an index of momentary ego-depletion obtained from a modified 5-item Depletion Scale adapted from Twenge et al. (2004). High Pollution is an indicator identifying individuals randomly assigned to complete the survey during a pollution episode. Alert is an indicator identifying individuals that received an additional pollution alert message 24-hours prior to completing the survey. Robust standard errors in brackets.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Table 1.7 presents OLS estimates of equation (1.1) for two measures of cognition. The first is an incentivised measure of cognitive ability (or fluid intelligence), obtained from a subset of Raven's Matrices (Bilker et al., 2012). The second, is a measure of state ego-depletion, obtained from a five-item depletion scale, adapted from Twenge et al. (2004). We find that the low and high pollution group perform close to identically in the cognitive ability task (difference of 0.05 correctly completed puzzles). Moreover, self-reported depletion appears to be slightly higher in the high pollution group, however, the difference is not statistically significant. When comparing high-pollution and high-pollution alert groups, we find comparably small differences, which are statistically indistinguishable from zero.

Table 1.8: Psychological health

	(1) Depressive Symptoms	(2) Negative Affect	(3) Positive Affect
High Pollution	0.079	-0.220	-0.697**
	(0.050)	(0.451)	(0.353)
High Pollution × Alert	-0.055	0.092	-0.067
	(0.042)	(0.384)	(0.310)
Constant	0.985***	13.958***	10.696***
	(0.136)	(1.342)	(1.027)
R^2	0.045	0.037	0.048
Observations	632	632	632

Note: OLS estimates of equation (1.1). In the first column, the dependent variable is an indicator for individuals that scored at least 10 on the CESD Scale, indicating the presence of depressive symptoms (Andresen et al., 1994). In the second and third columns, the dependent variables are measures of negative and positive affect, respectively, given by the sum of five negative (positive) affect items obtained from the PANAS-ISF (Thompson, 2007). High Pollution is an indicator identifying individuals randomly assigned to complete the survey during a pollution episode. Alert is an indicator identifying individuals that received an additional pollution alert message 24-hours prior to completing the survey. Robust standard errors in brackets.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Next, we explore the effect of air pollution on self-reported measures of depression and mood. All three measures were obtained from well-established and clinically verified screening questionnaires for depressive symptoms (Andresen et al., 1994) and positive and negative affect (Thompson, 2007). The pollution-health literature has produced substantial empirical evidence for a detrimental effect of air pollution on mental health, however, the majority of research has focused on long-term exposure. Our experimental design allows us to estimate the causal impact of acute exposure to extreme pollution. The results obtained from OLS regressions of equation (1.1) are presented in Table 1.8. We find that subjects in the high-pollution group were 7.9 percentage points more likely to be classified as having depressive symptoms (based on scoring 10 or higher on the 30-point CESD Scale) compared to the low-pollution group. However, the difference is not statistically significant at the 10% level. Moreover, the high-pollution group reported slightly lower negative affect scores and substantially lower positive affect scores as measured by the PANAS-ISF scale, the latter difference being statistically significant at the 5% level. Receiving a pollution alert message did not significantly change positive or negative affect but reduced the likelihood of depressive symptoms by 5.5 percentage points, which, again, does not reach statistical significance. The results indicate that, in line with the existing literature, exposure to air pollution has an immediate and statistically significant effect on depression and mood. This effect primarily manifests itself in acute reductions in positive affect on the day of the pollution episode but is also reflected by increases in the likelihood of reporting depressive symptoms, yet the latter is not statistically significant by conventional standards.

Table 1.9: General health

	(1) Physical Health	(2) General Health	(3) Sleep Quality
High Pollution	-0.152**	-0.092	-0.097
	(0.075)	(0.084)	(0.199)
High Pollution × Alert	0.110*	-0.010	0.226
	(0.066)	(0.072)	(0.165)
Constant	-0.362	2.515***	6.158***
	(0.233)	(0.237)	(0.534)
R^2	0.035	0.132	0.051
Observations	632	632	632

Note: OLS estimates of equation (1.1). In the first column, the dependent variable is an index of physical health, combining three indicators for self-reported presence of typical symptoms which may be affected by air pollution (cough, sore throat, stuffy nose). In the second column, the dependent variable is a self-reported measure of general health (reported on a scale of 1 to 5). In the third column, the dependent variable is a continuous measure of sleep quality *last night* (reported on a scale of 1 to 10). High Pollution is an indicator identifying individuals randomly assigned to complete the survey during a pollution episode. Alert is an indicator identifying individuals that received an additional pollution alert message 24-hours prior to completing the survey. Robust standard errors in brackets.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Finally, we examine differences in physical health, general health and sleep quality between the low and high pollution groups. Table 1.9 reports OLS estimates of equation (1.1). The dependent variable in Column (1) is an index combining the propensity of experiencing three types of symptoms potentially affected by pollution exposure (cough, sore throat and stuffy nose). The coefficient estimate is flipped so that a negative estimate reflects a deterioration in physical health. In line with our expectations, we observe that physical and general health are worse in the high-pollution group, compared to the low-pollution group. The difference in physical health is statistically significant at the 5% level, suggesting that subjects surveyed during the pollution episode were more likely to report that they had experienced or were experiencing the three symptoms. The dependent variable in Column (3) is a self-reported measure of last night's sleep quality. Empirical evidence has shown that air pollution may have an acute effect on sleep quality, thus influencing economic decision-making (Heyes & Zhu, 2019), however we find no evidence for this in our data. Subjects in the high-pollution group recalled their sleep-quality in the night prior to the survey to be only slightly worse than those surveyed during a low-pollution period (0.10 on a 10-point scale).

Interestingly, we observe that subjects that received a pollution alert message made slightly different evaluations of their physical health and sleep quality. Receiving an alert message increased self-reported physical health and last night's sleep quality, relative to the high-pollution group that received no additional pollution warning. However, only the increase in physical health is weakly statistically significant at the 10% significance level.

Figure 1.8 summarises the main results presented in this section and visually presents the standardised treatment effects of air pollution exposure and receiving an alert message on cognition and health outcomes. Effects are again presented in standard deviation units to allow direct comparison between outcomes.

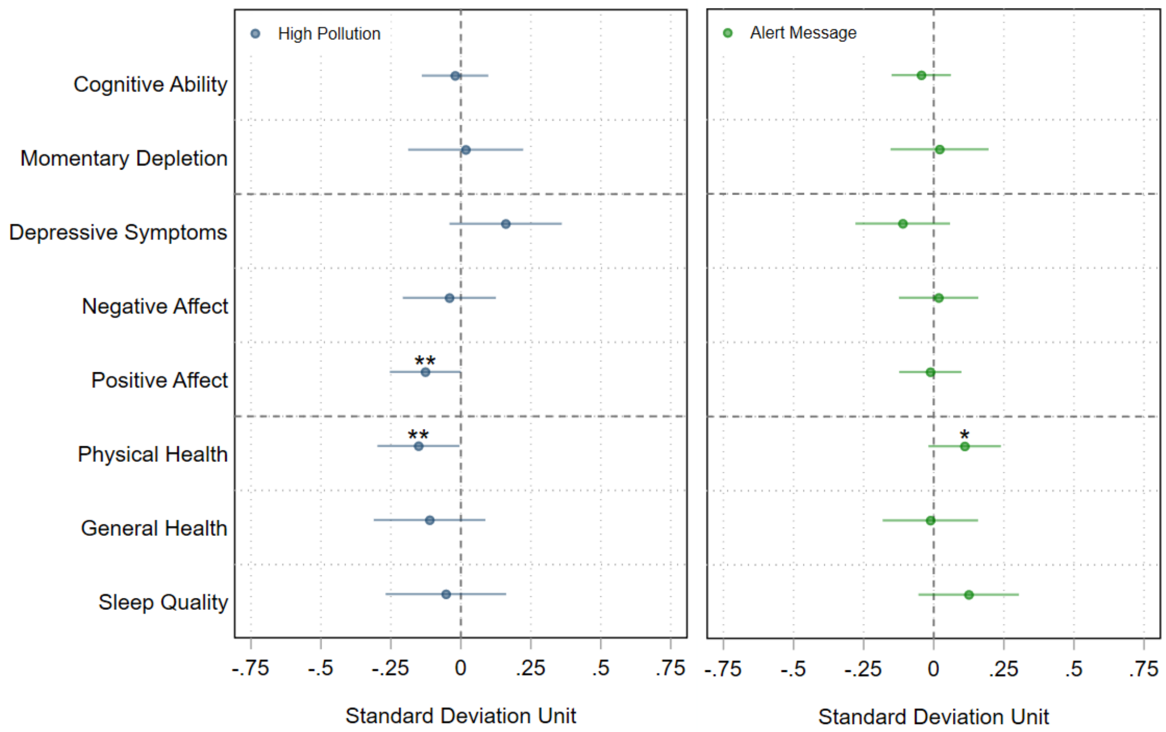

Figure 1.8: Results summary: Cognition & health outcomes

Note: The left panel displays the difference between the 'high-pollution' treatment group and the 'low-pollution' control group. The right panel shows the difference between 'high-pollution group' and 'high-pollution alert' group. All dependent variables were standardized (z-scored) on the mean prior to analysis. Error bars indicate 95% confidence intervals.

We find that acute pollution exposure had no effect on cognitive ability or self-reported depletion levels, with both estimates precisely estimated and close to zero. Turning to psychological health, we show that acute exposure to pollution reduced positive affect (0.15 SD, significant at the 5% level) and increased the likelihood of reporting depressive symptoms, yet the latter is not statistically different from zero. Moreover, pollution exposure had a detrimental effect on physical health, measured by three types of symptoms potentially related to pollution exposure (cough, sore throat and stuffy nose). Interestingly, this negative effect is offset by approximately the same magnitude if subjects received a pollution alert message (significant at the 10% level).

Taken together, the findings suggest that exposure to high levels of pollution has the expected negative impact on self-reported psychological well-being and physical health. In addition to the existing literature, which has often established a long-term effect, our findings suggest that the effect is also observable when people are subject to acute exposure.

1.4.4 Pollution alert messages protective behaviour

In this section we explore whether providing pollution warnings via direct message on WeChat influenced how pollution was perceived and whether it had an impact on protective behaviours. Our findings contribute to a growing literature exploring the efficacy of pollution alerts in encouraging protective behaviours (e.g. Delmas & Kohli, 2020).

To assess protective behaviour, we included a series of questions in the debriefing section of our experimental questionnaire (see Appendix Section C6). We asked participants to self-report whether they had checked pollution levels online, worn a mask, limited time outdoors or stayed indoors entirely on the day of the survey. For our analysis, we construct binary indicators identifying individuals that reported that they had engaged in the respective protective behaviour and utilise them as dependent variables in equation (1.1), estimated by OLS.[10] In this case, we are particularly interested in the coefficient on the interaction term (High Pollution × Alert) which indicates whether those individuals that received an alert message behaved differently from those that did not. Results are presented in Table 1.10.

[10]We acknowledge that a logit or probit model would be more appropriate in the case of binary dependent variables, however, we prefer OLS estimation which allows a direct interpretation of the coefficients.

Table 1.10: Protective behaviour

	(1) Check Online	(2) Wear Mask	(3) Limit Outdoors	(4) Stay Indoors	(5) Perceived Pollution
High Pollution	0.067	0.130***	0.348***	0.217***	2.888***
	(0.054)	(0.044)	(0.048)	(0.038)	(0.219)
High Pollution × Alert	0.030	0.049	0.052	-0.029	0.108
	(0.046)	(0.041)	(0.044)	(0.038)	(0.161)
Pollution Episode 2	-0.100**	-0.155***	-0.245***	-0.125***	-1.451***
	(0.049)	(0.041)	(0.047)	(0.038)	(0.174)
Constant	0.344***	0.029	0.074	-0.032	4.704***
	(0.131)	(0.113)	(0.127)	(0.099)	(0.498)
R^2	0.024	0.053	0.135	0.070	0.323
Observations	632	632	632	632	632

Note: OLS estimates of equation (1.1). In the first column, the dependent variable is a continuous variable capturing subjective perception of air pollution on the day of the survey. In the second column, the dependent variable is an indicator equal to 1 if the respondent checked pollution levels online on the day of the survey. The dependent variables in columns (3), (4) and (5) are indicators for three types of protective behaviour, wearing a anti-haze mask, limiting time outdoors or staying indoors entirely on the day of the survey. *High Pollution* is an indicator identifying individuals randomly assigned to complete the survey during a pollution episode. *Alert* is an indicator identifying individuals that received an additional pollution alert message 24-hours prior to completing the survey. *Pollution Episode 2* is an indicator identifying individuals that completed the survey during the second pollution episode. Robust standard errors in brackets.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results suggest that participants surveyed during a high pollution episode were significantly more likely to engage in protective behaviour (i.e., wear a mask, limit time outdoors and stay indoors), relative to individuals in the low-pollution group. However, we find no statistically significant differences between individuals that received a pollution alert and those that did not (High Pollution × Alert). While subjects in the alert condition were between 3 and 5 percentage points more likely to check pollution levels online, wear a mask or limit time outdoors, none of these differences are statistically significant at the 10% level. Moreover, subjects in the alert group were 2.9 percentage points less likely to stay indoors, arguably the most prohibitive protective behaviour. However, this difference is also not statistically distinguishable from zero.

The findings may be explained by the fact that providing a pollution alert message had no impact on the perceived level of air pollution (Column 5). As discussed in section 1.4.1, participants had a relatively accurate perception of air pollution and may take protective behaviour accordingly, regardless of having received an alert message or not. This explanation is further supported by the significant differences in protective behaviour between the first and the second pollution episode across all four behaviours. Participants surveyed during the second pollution episode perceived pollution to be significantly lower, and thus were less likely to engage in protective behaviours.

1.4.5 Perceived pollution

In this section we investigate whether the effect of pollution on behaviour may vary depending on how pollution is perceived. Previous literature in this area has suggested that perceived pollution mediates the effect of actual air pollution levels on unethical behaviour (R. Fehr et al., 2017; Gong et al., 2020; Lu et al., 2018). To further explore the role of perceived pollution in shaping social behaviour and economic decision-making, we classify individuals surveyed during the high pollution episodes into two groups: those that perceived pollution to be extremely high (i.e., above the 75th percentile of the response distribution, corresponding to those that reported air pollution to be equal to 9 or 10 on a scale of 1 to 10.) and those that perceived pollution to be less extreme (i.e., below the 75th percentile. Moreover, we exclude individuals who received a pollution alert.[11] We modify equation (1.1) so that we estimate differences between the low-pollution group and the high-pollution group that did not perceive pollution to be extremely high (β_1) and differences between the latter group and the high-pollution group that did perceive pollution to be extremely high (β_2).

$$Y_i = \beta_0 + \beta_1 High_i + \beta_2 High_i \times Perceived_i + \beta_3 EP2_i + \beta_4 H_i + S_i' + \varepsilon_i \tag{1.2}$$

We are particularly interested in the coefficient (β_2) on the interaction term $High_i \times Perceived_i$ as this estimate indicates whether decision-making differed between those that perceived pollution to be extreme and those that did not. Table 1.11 presents the results from this analysis for our primary outcomes for social behaviour.

We find significant differences in decision-making between the two groups for two of our six primary outcomes. Subjects that perceived pollution to be extremely high on the day of the

[11] While, as previously shown, our pollution alert message had no effect on perceived pollution or protective, it may have affected behaviour through other unobserved pathways. For the analysis presented here, we therefore focus on individuals whose perception or behaviour was in no way influenced by our experimental manipulation.

survey took on average 2.4 Yuan more from their counterpart in the variation of the Take Game with deterrence incentives and gave 1.56 Yuan less in the dictator transfer decision, compared to subjects who perceived air pollution to be less extreme, significant at the 5% and 1% level respectively.

Table 1.11: Perceived pollution: Primary outcomes

	(1) JOD	(2) Taking	(3) Taking (Det.)	(4) Giving	(5) Punish (Binary)	(6) Punish (Extent)
High	0.032	0.292	-0.292	0.582	0.031	0.434
	(0.046)	(0.776)	(0.856)	(0.436)	(0.061)	(0.304)
High × High Perceived	-0.006	1.491	2.408**	-1.561***	0.019	-0.458
	(0.062)	(1.023)	(1.204)	(0.544)	(0.081)	(0.374)
Constant	0.008	10.530***	9.933***	4.195***	0.247	0.696
	(0.166)	(2.522)	(2.730)	(1.359)	(0.200)	(0.914)
R^2	0.015	0.039	0.033	0.046	0.033	0.035
Observations	393	393	393	393	393	393

Note: OLS estimates of equation (1.2). The dependent variables are primary outcomes for social behaviour. The sample used to estimate all models excludes subjects that received a pollution alert warning. *High* is an indicator identifying individuals randomly assigned to complete the survey during a pollution episode. *High Perceived* is an indicator identifying individuals who subjectively perceived pollution to be very high (≥9 on a scale of 1-10). The interaction of both coefficients thus shows the difference between subjects who were in the high-pollution group and those who also perceived pollution to be extremely high. The coefficient for *High* indicates the difference between the low-pollution group and the subjects in the high-pollution group that did not perceive pollution to be extremely high. Robust standard errors in brackets.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Figure 1.9 visualises the mean taking and giving behaviour (and corresponding 95% confidence intervals) across the three groups. The average amount taken in the 'high-perceived' group was 11.86 Yuan, which is significantly higher than the amount taken in the 'low-pollution' (9.75 Yuan) and 'low-perceived' (9.45 Yuan) groups, at the 95% confidence level. Similarly, we observe that altruistic behaviour in the form of giving was substantially lower amongst individuals who perceived the pollution to be extremely high. Dictators in this group gave, on average, only approximately half the amount given by subjects who did not perceive pollution to be high (2.70 Yuan vs. 4.26 Yuan) and also significantly less than the control group, with both differences statistically significant at the 1% and 10% level, respectively.

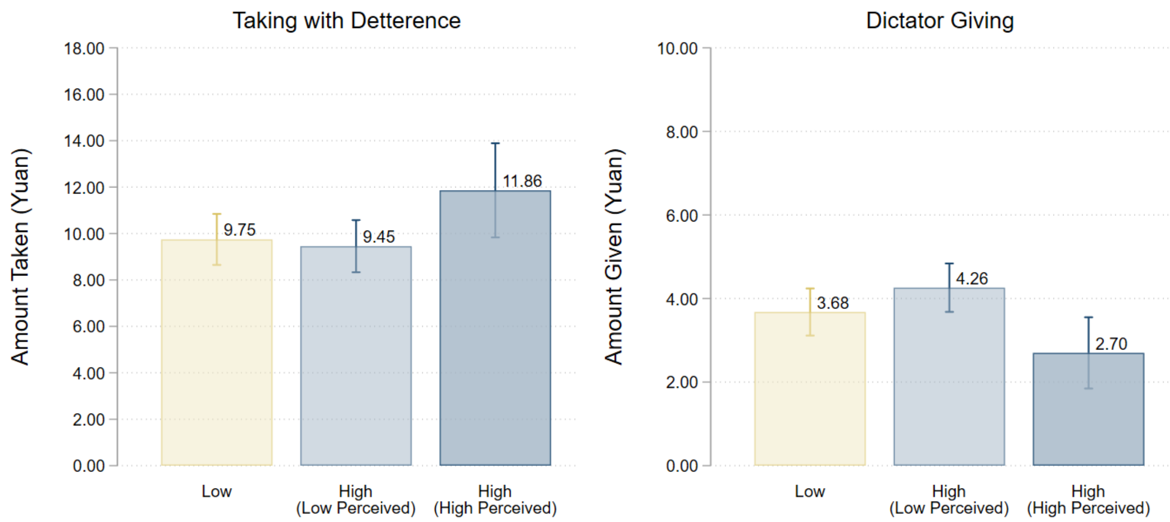

Figure 1.9: Taking and giving behaviour by pollution perceptions in the high-pollution group

Note: Error bars indicate 95% confidence intervals.

Table 1.12 shows estimates of equation (1.2) for the secondary outcomes which were obtained from incentivised tasks. We find no statistically differences in risk aversion, present bias or cognitive ability with respect to how the pollution levels were perceived. However, somewhat unexpectedly, we observe higher levels of patience amongst those participants who perceive pollution to be extremely high (Table 1.12, column 3). This finding stands in contrast to some of the recent evidence, which argues that temporary increases in intertemporal discounting (i.e., decreased patience) may explain the relationship between pollution and criminal behaviour (see e.g. Bondy et al., 2020).

Table 1.12: Perceived pollution: Secondary outcomes

	(1) Risk Aversion	(2) Present Bias	(3) Patience	(4) Cognitive Ability
High	-0.056	0.002	-0.004	-0.063
	(0.355)	(0.023)	(0.013)	(0.179)
High × High Perceived	-0.085	0.032	0.025**	0.060
	(0.498)	(0.026)	(0.011)	(0.236)
Constant	1.324	0.915***	0.987***	6.661***
	(1.160)	(0.069)	(0.029)	(0.620)
R^2	0.020	0.015	0.022	0.013
Observations	393	387	387	393

Note: OLS estimates of equation (1.2). The dependent variables include all secondary outcomes which were elicited using incentivised tasks. The sample used to estimate all models excludes subjects that received a pollution alert warning. *High* is an indicator identifying individuals randomly assigned to complete the survey during a pollution episode. *High Perceived* is an indicator identifying individuals who subjectively perceived pollution to be very high (≥9 on a scale of 1-10). The interaction of both coefficients thus shows the difference between subjects who were in the high-pollution group and those who also perceived pollution to be extremely high. The coefficient for *High* indicates the difference between the low-pollution group and the subjects in the high-pollution group that did not perceive pollution to be extremely high. Robust standard errors in brackets.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

It is important to note a limitation of this analysis, which implies that results must be interpreted with caution. In the absence of an external manipulation of perceived pollution, we cannot rule out that differences in behaviour between subjects that perceived pollution to be high and those that did not, is due to some unobserved factors (such as personality traits). Nonetheless, the estimated differences in social behaviour are striking and display a consistent pattern.

1.5 Discussion and conclusion

This chapter sets out a novel experimental design which exploits naturally occurring discontinuities in air pollution to experimentally examine the causal effect of acute air pollution n social behaviour, standard economic preferences and psychological well-being. The experiment combines elements of a lab-in-the-field design with online data collection procedures to imitate a setting in which respondents are randomly assigned to pollution exposure. This was achieved by using targeted surveys on both high and low-pollution days, which were carefully selected to differ only with respect to pollution levels.

Our results do not support the hypothesis that short-term exposure to elevated levels of air pollution affects anti-social behaviour, on average. While we observe a slight increase in anti-social behaviour under acute air pollution exposure, none of the differences are statistically significant at meaningful levels. Our findings thus do not align with Chew et al. (2021), the only other study exploring social behaviour in a controlled experimental setting using incentivised tasks. The authors find that exposure to "haze" (i.e., elevated levels of PM2.5) had a negative impact on student's other-regarding behaviour, making them less prosocial across several games. We acknowledge that all social-behaviour decisions in Chew et al. (2021) were incentivised with substantially higher stakes, which may explain the observed disparities in our findings. However, our results also stand in contrast to significant increases in risk aversion reported in Chew et al. (2021). We find a precisely estimated null effect of pollution exposure on risk aversion using a lottery-choice task with comparable incentivisation. Moreover, we find no significant acute effect of pollution on incentivised measures of present bias, patience and cognitive ability or self-control depletion, several plausible pathways through which pollution may affect anti-social behaviour.

Within the context of the broader quasi-experimental literature exploring the pollution-behaviour link, our results suggest that previous significant findings on the association between air pollution and (violent) crime rates (e.g. Bondy et al., 2020; Burkhardt et al., 2019) may be due to contextual factors which do not apply to our sample population of university students. For instance, social and contextual factors such as poverty or financial hardship might be more predominant in a population likely to commit crimes than in our sample of students. Individual factors such as a predisposition for risk taking may influence behaviour, as (baseline) risk seeking is one explanation of higher levels of criminal activity.

Nevertheless, focusing on the subgroup of individuals who actually perceived air pollution to be extremely severe on days with objectively high levels of air pollution, we find evidence that pollution increases anti-social behaviour in the form of 'taking behaviour' and simultaneously reduces altruism in a Dictator Game. Interestingly, these subjects take more Yuan from their counterparts in the variant of the Take Game in which there is a risk of being detected, which more accurately represents real-world criminal behaviour which contains an element of risk.

This finding thus aligns with the recent literature on pollution and criminal behaviour (Bondy et al., 2020; Burkhardt et al., 2019; Lu et al., 2020). However, our findings fail to support the hypothesis that increased discounting underlies changes in criminal behaviour brought about by pollution.

Nonetheless, these findings indicate that the impact of air pollution may be underestimated if measurement relies solely on objective metrics (R. Fehr et al., 2017). Recent research suggests that individuals' psychological experience of air pollution appears to influence real-world decision-making (R. Fehr et al., 2017; Gong et al., 2020; Lu et al., 2018). For instance, R. Fehr et al. (2017) find that perceived air pollution (i.e., air pollution appraisals) negatively impacts social behaviour in an organisational work context. Similarly, Lu et al. (2018) find that perceived pollution significantly increases unethical behaviour in the form of cheating. Gong et al. (2020) replicate and extend this research and conclude that "that the effect of air pollution on unethical behaviour is driven more by the subjective perception of increased air pollution rather than by actual increases in air pollution" (Gong et al., 2020, p.1045). Our results support these earlier findings by showing that social behaviour is impacted only for those participants who perceive pollution to be more extreme.

Moreover, our results indicate that acute exposure to extreme levels of air pollution negatively impacts psychological and physiological well-being. Participants surveyed during a pollution episode were, on average, significantly more likely to report lower levels of physical health (measured by common illness symptoms) and positive affect (or mood). Our findings are thus in close accordance with Zhang et al. (2017) who find that air pollution reduces hedonic happiness and increases the rate of depressive symptoms. Moreover, our findings are consistent with the broader economic and epidemiological literature on the adverse consequences of air pollution on mental well-being, happiness and depression, most of which has studied long-term exposure to air pollutants (e.g. Khan et al., 2019; Power et al., 2015; Pun et al., 2017; Xue et al., 2019; Zeng et al., 2019; Zhang et al., 2017). Our findings complement this literature by exploring the immediate short-term dimension of pollution exposure and provide evidence that even acute exposure to air pollution can have a direct negative impact on mental health. Our results thus provide support for Zheng et al. (2019) who find that pollution increases negative emotions (such as bad) mood expressed on Chinese social media, and Y. Li et al. (2019) who show that negative emotions occur when pollution levels surpass an AQI of 150 using psychophysical visual experiments.

Finally, our findings contribute to an emerging literature exploring the efficacy of pollution warnings and alerts (Delmas & Kohli, 2020; Graff Zivin & Neidell, 2009; Saberian et al., 2017;

Sexton & Timothy, 2016). First, we show that issuing pollution alerts via direct message on the day prior to a severe pollution episode were unsuccessful in encouraging additional self-reported protective behaviour (mask-wearing, checking pollution levels online, limiting time outdoors or staying indoors). However, we nonetheless document significant behavioural effects associated with providing alert messages. Specifically, we find that subjects in the high-pollution alert group were less likely to take from their counterparts and reported improved physical health, compared to the high-pollution group that received no alert. Interestingly, these findings suggest that some of the detrimental impacts of air pollution exposure were offset by receiving an alert message. Future research should explore this somewhat unexpected finding in more detail.

While our novel study design was successful in experimentally manipulating the level of air pollution that subjects were exposed to while completing the survey, it is important to discuss certain limitations. First, it remains unclear how cumulative pollution exposure prior to the survey date may confound our results. Participants in the high pollution groups were exposed to two days of increasing pollution prior to the day of the survey, whilst participants in the low-pollution group were exposed to the entire pollution episode as well as one day of low-pollution prior to the survey. If we assume that pollution has a more pro-longed (cumulative) physiological impact on the body and brain, participants in the low pollution group may not have fully "recovered" from the pollution episode, despite having had one day of clean air prior to completing the survey. Future research should employ larger samples to explore potential effects of short-term cumulative exposure and whether people behave differently after longer periods of "recovery".

Second, we acknowledge that our analysis is based on a relatively small sample size, which may be underpowered to detect an effect on behaviour even if an effect is present. We may however argue that small effect sizes, as observed in our data (such as a 3-percentage point increase in destructive behaviour), are not of particular economic significance, even if they were found to be statistically significant with a larger sample size. We thus believe that Type II error is not a significant cause of concern in our study.

Third, we must caution with respect to the external validity of our findings, which relies on a sample of students who permanently live in Beijing. Students in Beijing may be familiar with extreme levels of air pollution, and therefore habituation may attenuate the effects. For example, if we were to conduct the same experiment with tourists visiting Beijing from rural (low-polluted) regions, we may come to very different conclusions. For instance Y. Li et al. (2019) found that people living in the UK showed a stronger negative response to viewing images of extreme pollution than Chinese observers. Moreover, our student sample is clearly not representative of the general population, a common drawback of experimental research that utilises student subjects. However, there is increasing evidence that student samples are appropriate for studying human social behaviour (Exadaktylos et al., 2013; Falk et al., 2013). Moreover, if air pollution were to affect fundamental aspects of decision-making, independent of contextual factors, this should also be detectable in a student sample. In this regard, our findings point to the importance of contextual factors which may interact with air pollution to bring about changes in social behaviour and economic preferences.

In sum, our results suggest that people's mood is negatively affected on polluted days, however, not enough to significantly impact decision-making in our sample. Nonetheless, we present suggestive evidence that pollution exposure raises anti-social behaviour and decreases altruistic behaviour on polluted days amongst individuals who perceived pollution levels to be extremely high. Future research should attempt to experimentally manipulate perceived pollution to further probe the robustness of our findings. Moreover, future research should utilise larger non-student samples to further explore the link between pollution and human decision-making. We hope that our experimental design provides a methodological foundation for future work and will stimulate further innovations in research design to strengthen experimental identification and causal inference.

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Appendix

Appendix 1.A Additional tables and figures

Tables

Table 1.A1: Survey modules

Group	Measure	Description	Variable construct
Anti-social Behaviour	Joy of Destruction (Abbink & Herrmann, 2011)$	Binary decision to anonymously destroy a matched player's endowment as a measure of nastiness.	Binary variable: Equal to 1 if the participant decides to destroy their counterpart's endowment at a cost to him/her-self.
	Take Game (Schildberg-Hörisch & Strassmair, 2012)$	Amount of endowment taken from a matched player as a measure of theft.	Amount (¥) taken from other player's endowment
	Take Game with Deterrence (Schildberg-Hörisch & Strassmair, 2012)$	Amount of endowment taken from a matched player with a 40% chance of detection resulting in loss of endowment, as a measure of theft with risk.	Amount (¥) taken from other player's endowment
Norm-enforcement	Third-party punishment game (E. Fehr & Fischbacher, 2004)$	Amount of costly punishment imposed on a matched player based on the amount transferred by the matched player in a dictator game.	Binary variable: Equal to 1 if a participant punishes any amount when the dictator transfers zero. Extent variable: Amount (¥) punished at a cost ratio of 1¥ for every 3¥ deducted.
Pro-social Behaviour	Dictator Game (E. Fehr & Fischbacher, 2004)$	Amount of endowment transferred to a matched player (decision observed by third party).	Amount (¥) transferred to matched recipient.
Economic Preferences	CRRA coefficient (Eckel & Grossman, 2002)$	Choice between six lotteries (50/50 odds) increasing in variance, absolute pay-off and riskiness.	Coefficient of relative risk aversion midpoints (CRRA)
	Present Bias (Andreoni et al., 2015)$	Individual β parameter derived from 24 budget lines across 4 timeframes	Present Bias (individual beta parameter).
	Time Discounting (Andreoni et al., 2015)$	Individual δ parameter derived from 24 budget lines across 4 timeframes	Discount rate (individual delta parameter)
Cognition & Health	Raven's Matrices (Bilker et al., 2012)$	Cognitive ability measured by the number of correctly completed puzzles (out of 9).	Score between 0 and 9.
	Depletion	Five-item depletion scale adapted from Twenge et al. (2004).	Score between -1.4 and +2.4
	Depression (Andresen et al., 1994)	Depression score calculated using the Centre for Epidemiological Studies Depression Scale Short-form (CESD-10).	Binary variable: Equal to 1 if depression score is greater than 10 (on scale of 0 - 30)
	Positive & Negative Affect (Thompson, 2007)	Assessment of mood on the day of the survey using the international Short-form of the Positive and Negative Affect Schedule (PANAS-ISF)	Positive & negative affect score between 5 and 25 (sum of five postive/negative items).
	Physical health	Self-assessed physical health status based on common symptoms (cough, sore throat, stuffy nose)	Index (average of standardised variables)
	General health	Self-assessed general health status	Likert scale between 1 and 5
	Sleep Quality	Self-assessed sleep quality in the night before the survey day	Self-report scale between 0 and 10

Note: $ Incentivised tasks

Table 1.A2: Risk preferences: Lottery choice schedules

Nr.	50% chance to get	50% chance to get
1	56¥	56¥
2	48¥	72¥
3	40¥	88¥
4	32¥	104¥
5	24¥	120¥
6	4¥	140¥

Note: Based on Eckel and Grossman (2002)

Table 1.A3: Time preferences: Budget lines

	Time Frame				Options					
Budget #	#1	#2	#3	#4	1	2	3	4	5	6
1, 8, 13, 20	Today	Today	5 weeks	5 weeks	95 ¥	76 ¥	57 ¥	38 ¥	19 ¥	0 ¥
	5 weeks	9 weeks	10 weeks	14 weeks	0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
2, 9, 14, 21	Today	Today	5 weeks	5 weeks	90 ¥	72 ¥	54 ¥	36 ¥	18 ¥	0 ¥
	5 weeks	9 weeks	10 weeks	14 weeks	0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
3, 15	Today	5 weeks			85 ¥	68 ¥	51 ¥	34 ¥	17 ¥	0 ¥
	5 weeks	10 weeks			0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
4, 16	Today	5 weeks			80 ¥	64 ¥	48 ¥	32 ¥	16 ¥	0 ¥
	5 weeks	10 weeks			0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
5, 17	Today	5 weeks			70 ¥	56 ¥	42 ¥	28 ¥	14 ¥	0 ¥
	5 weeks	10 weeks			0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
6, 18	Today	5 weeks			55 ¥	44 ¥	33 ¥	22 ¥	11 ¥	0 ¥
	5 weeks	10 weeks			0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
7, 19	Today	5 weeks			100 ¥	80 ¥	60 ¥	40 ¥	20 ¥	0 ¥
	9 weeks	14 weeks			0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
10, 22	Today	5 weeks			75 ¥	60 ¥	45 ¥	30 ¥	15 ¥	0 ¥
	9 weeks	14 weeks			0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
11, 23	Today	5 weeks			60 ¥	48 ¥	36 ¥	24 ¥	12 ¥	0 ¥
	9 weeks	14 weeks			0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥
12, 24	Today	5 weeks			45 ¥	36 ¥	27 ¥	18 ¥	9 ¥	0 ¥
	9 weeks	14 weeks			0 ¥	20 ¥	40 ¥	60 ¥	80 ¥	100 ¥

Note: Based on Andreoni et al. (2015), adapted from Y. Chen et al. (2019).

Table 1.A4: Balance checks

Variable	(1) Mean Control	(2) Mean High	(3) Mean High Alert	(4) High vs Control	(5) High Alert vs Control	(6) High vs High Alert
Female	0.78	0.78	0.77	-0.01	-0.01	0.00
	(0.41)	(0.42)	(0.42)	(0.04)	(0.04)	(0.04)
University District	1.60	1.63	1.60	0.03	0.00	0.03
	(0.85)	(0.86)	(0.85)	(0.09)	(0.09)	(0.08)
Year of Study	2.63	2.55	2.57	-0.09	-0.06	-0.03
	(1.18)	(1.13)	(1.18)	(0.12)	(0.12)	(0.11)
Airpollution impacts my health	3.93	3.87	3.90	-0.06	-0.03	-0.03
	(0.92)	(0.95)	(0.89)	(0.10)	(0.09)	(0.09)
Rural Hukou	0.18	0.22	0.20	0.04	0.02	0.03
	(0.39)	(0.42)	(0.40)	(0.04)	(0.04)	(0.04)
Only Child	0.69	0.64	0.64	-0.04	-0.05	0.01
	(0.47)	(0.48)	(0.48)	(0.05)	(0.05)	(0.04)
Age	19.89	19.87	19.94	-0.02	0.05	-0.07
	(1.59)	(1.42)	(1.62)	(0.15)	(0.16)	(0.14)
Years living in Beijing	6.23	5.92	6.57	-0.31	0.34	-0.65
	(6.88)	(6.76)	(6.99)	(0.70)	(0.70)	(0.64)
Health Status at Baseline	3.72	3.67	3.87	-0.05	0.15*	-0.20**
	(0.86)	(0.78)	(0.89)	(0.08)	(0.09)	(0.08)
Predicted health status this term	7.25	7.19	7.37	-0.06	0.12	-0.18
	(1.97)	(1.84)	(1.92)	(0.19)	(0.20)	(0.17)
Egalitarian Preferences	0.33	0.31	0.34	-0.02	0.01	-0.03
	(0.47)	(0.46)	(0.47)	(0.05)	(0.05)	(0.04)
Favourable Inequality	0.19	0.18	0.20	-0.01	0.01	-0.02
	(0.39)	(0.39)	(0.40)	(0.04)	(0.04)	(0.04)
Behindness Averse	0.55	0.56	0.63	0.00	0.07	-0.07
	(0.50)	(0.50)	(0.48)	(0.05)	(0.05)	(0.05)
Cooperation (¥ invested in PGG)	4,511.45	4,395.18	4,474.40	-116.27	-37.04	-79.22
	(3,110.62)	(3,339.30)	(3,230.43)	(331.37)	(321.49)	(304.35)
Perceived air quality Beijing	2.64	2.56	2.52	-0.09	-0.12	0.03
	(0.95)	(0.85)	(0.83)	(0.09)	(0.09)	(0.08)
Perceived air quality hometown	3.46	3.57	3.48	0.10	0.01	0.09
	(1.24)	(1.09)	(1.14)	(0.12)	(0.12)	(0.10)
Perceived air quality current residence	2.81	2.75	2.72	-0.06	-0.09	0.03
	(0.93)	(0.82)	(0.86)	(0.09)	(0.09)	(0.08)
Predicted air quality this term	5.66	5.54	5.68	-0.12	0.03	-0.14
	(1.90)	(1.80)	(1.74)	(0.19)	(0.18)	(0.16)
Airpollution impacts my health	3.93	3.87	3.90	-0.06	-0.03	-0.03
	(0.92)	(0.95)	(0.89)	(0.10)	(0.09)	(0.09)
Air purifier in dorm room	0.09	0.14	0.10	0.05	0.01	0.04
	(0.29)	(0.34)	(0.30)	(0.03)	(0.03)	(0.03)
Observations	166	227	239	393	405	466

Note: This table presents balance checks of sample characteristics between the High, High-Alert and Low pollution control group. Columns (1) to (3) display the sample mean for each group, respecitvely. Columns (4) and (5) display the differences in the mean of each treatment group compared to the control mean. Column (6) compares the means of both treatment groups to each other. Significance stars on columns (4) to (6) indicate whether differences in means are statistically significant based on p-values obtained from two-sample t-tests.

Table 1.A5: Balance checks: Baseline preferences

Variable	(1) Mean Control	(2) Mean High	(3) Mean High Alert	(4) High vs Control	(5) High Alert vs Control	(6) High vs High Alert
Trust (¥)	45.21	47.65	50.93	2.44	5.72**	-3.29
	(24.58)	(26.50)	(24.66)	(2.63)	(2.49)	(2.37)
Cooperation (¥ invested in PGG)	4,511.45	4,395.18	4,474.40	-116.27	-37.04	-79.22
	(3,110.62)	(3,339.30)	(3,230.43)	(331.37)	(321.49)	(304.35)
Egalitarian Preferences	0.33	0.31	0.34	-0.02	0.01	-0.03
	(0.47)	(0.46)	(0.47)	(0.05)	(0.05)	(0.04)
Favourable Inequality	0.19	0.18	0.20	-0.01	0.01	-0.02
	(0.39)	(0.39)	(0.40)	(0.04)	(0.04)	(0.04)
Behindness Averse	0.55	0.56	0.63	0.00	0.07	-0.07
	(0.50)	(0.50)	(0.48)	(0.05)	(0.05)	(0.05)
Risk Aversion (CRRA Midpoint)	2.91	3.12	2.99	0.21	0.08	0.13
	(2.79)	(2.80)	(2.82)	(0.29)	(0.28)	(0.26)
Willingness to Compete	12.72	12.60	12.78	-0.12	0.06	-0.17
	(4.46)	(4.20)	(3.72)	(0.44)	(0.41)	(0.37)
Depressive Symptomns (Yes = 1)	0.41	0.42	0.44	0.01	0.03	-0.03
	(0.49)	(0.49)	(0.50)	(0.05)	(0.05)	(0.05)
Health Status at Baseline	3.72	3.67	3.87	-0.05	0.15*	-0.20**
	(0.86)	(0.78)	(0.89)	(0.08)	(0.09)	(0.08)
Observations	166	227	239	393	405	466

Note: This table presents balance checks of baseline preferences and behaviour between the High, High-Alert and Low pollution control group. Columns (1) to (3) display the sample mean for each group, respecitvely. Columns (4) and (5) display the differences in the mean of each treatment group compared to the control mean. Column (6) compares the means of both treatment groups to each other. Significance stars on columns (4) to (6) indicate whether differences in means are statistically significant based on p-values obtained from two-sample t-tests.

Table 1.A6: Perceived pollution

	Rate Today's Air Pollution (0-10)			
	(1) General	(2) Visual	(3) Smell	(4) Media
High Pollution (Ep.1)	2.889***	2.085***	1.264***	0.936***
	(0.227)	(0.277)	(0.292)	(0.277)
High Pollution + Alert (Ep.1)	2.996***	2.465***	1.527***	1.625***
	(0.221)	(0.262)	(0.269)	(0.252)
High Pollution (Ep.2)	1.436***	0.650*	0.648*	-0.179
	(0.275)	(0.339)	(0.355)	(0.344)
High Pollution + Alert (Ep.2)	1.547***	1.193***	0.784**	0.026
	(0.264)	(0.336)	(0.369)	(0.350)
Airpollution impacts my health	0.219***	0.231**	0.316***	0.305***
	(0.084)	(0.102)	(0.108)	(0.104)
Perceived air quality Beijing	-0.244**	0.014	-0.001	0.236**
	(0.096)	(0.114)	(0.120)	(0.114)
Air purifier in dorm room	-0.185	-0.549*	-0.186	-0.270
	(0.235)	(0.283)	(0.311)	(0.288)
Years living in Beijing	-0.015	0.010	-0.007	0.002
	(0.011)	(0.013)	(0.015)	(0.013)
Female	-0.361**	0.184	0.252	0.296
	(0.179)	(0.237)	(0.257)	(0.228)
Outdoors	0.087	-0.020	0.513	-0.034
	(0.245)	(0.305)	(0.345)	(0.325)
Constant	4.704***	4.571***	3.400***	4.258***
	(0.498)	(0.598)	(0.608)	(0.593)
R^2	0.323	0.170	0.072	0.107
Observations	632	632	632	632

Note: Table presents estimates of simple linear regressions of perceived pollution measures (general, visual, smell and media coverage) on indicators for high and high-alert treatment groups for both pollution episodes separately (low-pollution control group is the omitted category) and a vector of controls. The Depedendent variables are self-reports (on a scale of 0 - 10) of perceived pollution on the day of the survey.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.A7: Risk and time preferences

	(1) Risk Aversion	(2) Present Bias	(3) Patience
High Pollution	-0.038	-0.005	0.0005
	(0.231)	(0.011)	(0.0002)
High Pollution × Alert	-0.160	0.007	-0.0004
	(0.195)	(0.012)	(0.0003)
Constant	1.133***		
	(0.648)		
Observations	632	622	622

Note: Table presents robustness checks for risk and time preferences. The first column shows estimates of equation (1.1) using an interval regression where the dependent variable is the CRRA interval (upper and lower bounds) obtained from a Lottery Choice Task (Eckel & Grossman, 2002). Columns two and three present differences in means between *aggregate* beta (present bias) and delta (patience) parameter estimates derived from a Convex Time Budgets task, following Andreoni et al. (2015). Standard errors (in parenthesis) are computed using a standard two-tailed t-test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 1.B Reminder and alert messages

Notification messages to participants 24-hours prior to survey launch

No warning:

'Dear participant, you are receiving this message because you participated in the first round of [Experiment Name] in October. Please be informed that we will send you the link for the second survey **tomorrow at 5pm**. You will have until **midnight tomorrow** to complete the survey. You will receive bonus money upon successful completion in time. We greatly appreciate your time and thank you for your cooperation in advance'

Alert:

'Dear participant, you are receiving this message because you participated in the first round of [Experiment Name] in October. Please be informed that we will send you the link for the second survey **tomorrow at 5pm**. You will have until **midnight tomorrow** to complete the survey. You will receive bonus money upon successful completion in time. We greatly appreciate your time and thank you for your cooperation in advance'

Please note that we expect very high pollution levels tomorrow in Beijing. High air pollution can have a significant impact on your health and we recommend taking appropriate measures to protect yourselves.

Appendix 1.C Experimental protocol

Appendix C contains the experimental protocol, which is available online at: https://drive.google.com/file/d/1n1wWlkxMC58qQskApC5vSSeNVdgrREx_/view?usp=sharing

Chapter 2

Does flood and heatwave experience shape climate opinion? causal evidence from flooding and heatwaves in England and Wales

2.1 Introduction

The UK has set itself ambitious climate targets and strives to be an international leader in climate change policy.[1] However, in order to reach these objectives, pervasive behavioural and societal changes as well as widespread public support for increasingly ambitious mitigation and adaptation policies will be required. Despite widespread belief in the existence of climate change and emerging climate activism, climate change remains a psychologically distant issue for some (Steentjes et al., 2017). Psychological distance refers to the belief that climate change is occurring in geographically distant regions, happening further into the future, and affecting different social groups (Spence et al., 2012; Taylor, Dessai, et al., 2014). A closer look at specific attitudes towards climate change shows that some scepticism and uncertainty remain amongst the UK population, especially with respect to potential impacts (Hagen et al., 2016; Taylor et al., 2017). A lack of personal relevance and perceived risk due to psychological distance has been identified as a major threat to public engagement around the issue. However, it has been postulated that highlighting the proximal consequences of climate change may

[1] https://www.gov.uk/government/news/uk-enshrines-new-target-in-law-to-slash-emissions-by-78-by-2035

increase engagement and motivation to act upon climate change (Demski et al., 2017; Leviston et al., 2014; Loy & Spence, 2020; Reser et al., 2014; Spence et al., 2011).

The appeal of proximising climate change as a policy tool to motivate action and engagement has sparked interest into whether personal experience with extreme weather events is related to heightened awareness and concern around climate change. Following recent advances in attribution science, there is mounting evidence that anthropogenic warming is linked with increasing intensity, frequency and duration of extreme weather events around the globe (IPCC, 2021). In the UK, future heatwaves and flooding pose a particular threat to individuals and the economy (Slingo, 2021). Climate projections suggest that summer heatwaves could occur every other year by the mid-21st Century (Slingo, 2021) and temperatures exceeding 40°C could be reached every three-and-a-half years by 2100 (Christidis et al., 2019), posing a significant threat to public health. Moreover, expected annual damages from flooding could nearly double by 2050, if warming follows a 4°C pathway (Sayers et al., 2020). However, processing abstract statistical information on the risks associated with future climate change impacts is cognitively demanding and requires substantial effort. In contrast, experiential learning is intuitive and involves rapidly occurring affective, associative and automatic processes (Ogunbode et al., 2020). Moreover, experiencing extreme weather events plausibly attributable to climate change can increase the saliency of negative consequences for a specific place that people care about, increasing personal relevance and perceived risk and eliciting a state of aversive arousal (Brügger et al., 2015). [2] This, in turn, should motivate private adaptation and mitigation behaviour as well as increased support for government policies.

This compelling argument, founded in psychological and economic theories, has inspired both empirical and experimental research into the proposed relationship between 'Climate Change Proximity' and beliefs and engagement.[3] However, the existing empirical evidence is mixed (Howe, 2021; Howe et al., 2019; Sisco, 2021). Many empirical papers looking at real-world climate proximity (i.e., directly experiencing extreme weather events) have suffered from methodological drawbacks and thus have not been able to establish a causal link (Howe, 2021). The lack of regional disaggregation in climate change opinion data and the reliance on correlational research designs have been identified as some of the most common pitfalls of past studies (Howe, 2021; Marquart-Pyatt et al., 2014; McCright et al., 2016). Another key issue relates to potential selection bias arising from residential sorting, which has been

[2]Aversive arousal refers to an unpleasant emotional state arising from the outlook of negative impacts for a certain place, or people implicated by that place, as a result of climate change.

[3]See Howe et al. (2019) and Sisco (2021) for reviews of empirical evidence and Schuldt et al. (2018) for experimental research. See Brügger et al. (2021) for a review of psychological processes underlying the association between extreme events and beliefs.

insufficiently addressed in the extant literature (Howe, 2019).[4] While most of the previous work has focused on finding any detectable relationship between extreme weather events and climate change attitudes, only few studies are able to provide insights into when and how personal experience has an impact (Brügger et al., 2021). Moreover, the primary focus of the existing literature has been on climate change attitudes and beliefs, while less is known about how behavioural outcomes (such as pro-environmental behaviours) respond to extreme weather experiences. Ultimately, changes in individual pro-environmental behaviour will play an important role in tackling climate change (Steg, 2018).

In this chapter, we investigate whether an individual's climate change risk perceptions, beliefs and pro-environmental behaviour change after they have experienced extreme weather events, specifically, flooding and heatwave events which affected large parts of the UK between 2009 and 2020. We exploit the geographic variation in flood and heatwave exposure combined with a propensity score matching and differences-in-differences identification strategy to estimate the causal effect of extreme event experience on three important domains of climate change attitudes: (1) risk perceptions towards future climate change impacts, (2) climate change concern and (3) self-reported pro-environmental behaviour. We utilise climate change opinion data from the UK Household Longitudinal Survey (UKHLS), a large-scale UK household panel survey covering approximately 40,000 households. For this project, we were granted access to the 'secure access' version of the dataset (SN 6676), which provides geo-referenced location information for survey participants (University of Essex, 2020). This allows us to spatially link individuals' exact household locations with high resolution flood outlines and temperature grids.

This chapter aims to address several important gaps found in the relevant literature. First, we utilise a difference-in-differences (DID) identification strategy to provide causal evidence for the relationship between extreme weather events and climate change attitudes in the UK. We strengthen our causal identification by introducing a complementary propensity score matching approach to minimise selection bias from unobserved residential sorting. Second, we present the most spatially precise analysis to date, by drawing on geo-referenced individual-level climate change opinion data, allowing us to observe the exact household location of each survey respondent. We establish extreme event exposure by linking this data with high-quality spatial data of flood events and high-resolution temperature grids using GIS techniques. Moreover, the panel structure of our opinion data allows us to control

[4]In this context, selection bias occurs if individuals self-select into or away from areas which are more likely to experience extreme events. If people that live within proximity to extreme weather events systematically differ from the comparison group (i.e., people living further away), causal inference from both cross-sectional and longitudinal designs can be limited.

for unobserved individual characteristics (e.g. personality traits), which may be important determinants of climate change perceptions. Third, we improve on previous research by exploring a nuanced set of questions spanning three important dimensions of climate change attitudes: (1) climate change risk perceptions, (2) climate change concern and (3) self-reported pro-environmental behaviour. Our spatially detailed analysis allows us to provide some novel insights into how and under what circumstances personal experience can have an impact on these outcomes. Finally, our study focuses on the two types of extreme weather events most relevant in the UK context, namely flooding and heatwaves. Our findings thus offer interesting insights into how attitudes and behaviour might respond to increasingly frequent weather events in the UK and give rise to important policy implications.

We show that, on average, personal experience with both flooding and heatwave events increase climate change risk perceptions but have no effect on climate change concern and stated pro-environmental behaviour. Moreover, we document a proximity effect for flooding and a frequency effect for heatwaves. The closer a flood occurs to a household, the more pronounced its effect on risk perceptions. Moreover, experiencing multiple heatwave events increases both climate change concern and pro-environmental behaviour, which has important implications given the increasing frequency of extreme heat events in the UK.

2.2 Related literature

2.2.1 Personal experience and climate change opinion

A growing body of social science literature is interested in the link between personal experience with climate change variations and attitudes towards climate change. Numerous studies have assessed the relationship by linking spatially disaggregated opinion data with objective weather data (Howe et al., 2019; Sisco, 2021). Climate parameters under investigation have included long-term climatic patterns and trends (Shao, 2017) as well as seasonal, monthly and daily temperature anomalies relative to a statistically constructed baseline (Bergquist & Warshaw, 2019; Bohr, 2017; Deryugina, 2013; Marlon et al., 2021; Shao, 2016). A related strand of literature has produced ample evidence for a link between climate change beliefs and short-run weather fluctuations, which has been termed the "local warming effect" (Damsbo-Svendsen, 2020; Joireman et al., 2010; Zaval et al., 2014). The local warming effect refers to the phenomenon that individuals are more likely to believe in the existence of global warming if interviewed on a hot day, in contrast to cold days. The majority of studies find that immediate and salient local weather conditions directly influence people's beliefs (Sugerman et al., 2021).

A further group of studies focuses specifically on how personal experience of extreme weather events relate to climate change beliefs, concerns and risk perceptions.[5] In contrast to long-term temperature trends (which are difficult to detect) and short-term temperature fluctuations (which do-not accurately represent a changing climate), extreme weather events are often perceived as embodying highly salient physical manifestations of anthropogenic climate change which may be more easily attributable to climate change. Past research has primarily focused on the US and largely exploits the exogenous variation in extreme events as a form of natural experiment. The majority of these studies find a positive yet moderate effect of extreme weather phenomena on beliefs and attitudes, which diminishes with time (Albright & Crow, 2019; Carlton et al., 2016; Dai et al., 2015; Deng et al., 2017; Hazlett & Mildenberger, 2020; Konisky et al., 2016; Ray et al., 2017; Sisco et al., 2017; Zanocco et al., 2019).

In the European context, research has primarily focused on heatwave exposure (Frondel et al., 2017; Larcom et al., 2019) and extreme flooding events (Demski et al., 2017; Frondel et al., 2017; Osberghaus & Fugger, 2018; Spence et al., 2012; Whitmarsh, 2008). Research assessing the link between flood experience and climate change beliefs in the UK has produced somewhat mixed results. Early studies in the UK found that flood experience did not significantly affect climate change belief (Whitmarsh, 2008). Later work by Spence et al. (2012) found that flood experience was positively related to the willingness to save electricity. Relatedly, flood experience has been linked to higher flood risk perception (Frondel et al., 2017) as well as household mitigation and adaptation behaviour (Osberghaus, 2017; Osberghaus & Demski, 2019). In a case-study of the severe 2013/2014 UK winter floods, Demski et al. (2017) found further evidence for heightened climate change concern and agency amongst flood victims, using subjective flood experience data. More recently, two case-studies in Germany have found that flood experience leads to heightened climate change concern (Osberghaus & Fugger, 2018) and may even encourage climate change engagement (Osberghaus & Demski, 2019). In contrast, heatwave exposure has been shown to make climate change more salient (Frondel et al., 2017; Taylor, De Bruin, et al., 2014), but has no effect on pro-environmental behaviour (Larcom et al., 2019).

A recent working paper by Rüttenauer (2021) explores the effect of both flood and heatwave exposure on climate change belief and behaviour using data from the UK. The author concludes that experiencing extreme weather events is associated with an increase in climate change belief, but has no effect on pro-environmental behaviour. While this study utilises individual-level panel data linked with objective measures of extreme weather events, it does not account

[5] Extreme weather events are commonly defined as significant unusual weather phenomena that have sufficient intensity to cause damages and/or disruption (Konisky et al., 2016).

for a range of potential endogeneity problems such as residential sorting both prior and during the study period. One challenge with estimating the causal effect of extreme event exposure on climate change beliefs is that they do not occur randomly across geographic locations. While this is an obvious limitation for cross-sectional designs, it may also be of concern in longitudinal (DID) designs. If unobserved residential sorting leads to systematic differences between treatment and control groups, this may potentially violate the assumption of parallel trends, crucial to empirical identification (Bakkensen & Ma, 2020). Furthermore, Rüttenauer (2021) explores only a subset of climate change attitudes collected in the survey data it uses. The UKHLS provides a host of additional climate change perceptions questions, which we utilise to construct an index of climate change concern. Finally, Rüttenauer (2021) relies on population weighted centroids of small-area geographical units as a proxy for participants' household location when assigning individuals to treatment and control groups, a common approach if the exact household location is unknown. As our analysis will show, the effect of flood exposure is highly sensitive to flood proximity, suggesting that inaccuracies in participants' locations may weaken internal validity. In contrast, our 'secure access' dataset allows us to observe individuals' exact geographic location, providing the most geographically accurate analysis to date.

Taken together, the review of the recent literature reveals that there is increasing evidence for a link between personal experience and climate change attitudes. However, the wide variety of different research designs, differences in spatial and temporal scales, inconsistencies in measurement of climate change opinions, and the lack of methodological rigour limit the generalisability of the existing body of research (Howe, 2021; Howe et al., 2019). Moreover, very few studies have been able to provide evidence about when and how experiences are likely to trigger different types of cognitive, emotional and behavioural responses (Brügger et al., 2021). This study addresses all of the previously discussed methodological limitations, which allows us to provide causal insights and additionally explore several potential mechanisms through which personal experience may affect climate change attitudes in the UK.

2.2.2 Mechanisms and hypotheses

There are several potential mechanisms through which personal experience of weather events could influence climate change perceptions, theoretically founded in both economics and cognitive science. In first instance, people may update their prior beliefs and behaviour through a Bayesian updating process (Deryugina, 2013; Druckman & Mcgrath, 2019; Larcom et al., 2019). According to Bayes' Rule, climate change belief is a function of prior beliefs combined with new available information from an observed signal (Holt & Smith, 2009).

If extreme weather is interpreted as new evidence for climate change, this should lead to a stable change in belief in favour of climate change.[6] While Bayesian updating provides a plausible theoretical starting point, there are likely to be numerous complementary and alternative psychological processes that underlie the complex relationship between experience of extreme weather events and climate change beliefs and thereby influence the updating process (Brügger et al., 2021).[7] For instance, experiencing negative affect associated with climate change may be a potential pathway through which personal experience interacts with climate change risk perceptions (van der Linden, 2014). Moreover, the importance of extreme event experience depends in part on its location, intensity, duration, type and how it is interpreted (Marlon et al., 2018), as well as the degree of cognitive attribution (Ogunbode et al., 2019; van der Linden, 2014). If no conscious link is drawn between the extreme event and climate change, Bayesian updating will not occur.

Moreover, numerous other heuristics and biases may be at work, leading to a departure from the Bayesian updating norm (B. G. Charness & Levin, 2005; G. Charness et al., 2007). For instance, people may be subject to an "availability" heuristic, under which they give greater weight to recent salient events when computing the probability of an event to occur (Tversky & Kahneman, 1973). Recent research finds support for this hypothesis, showing that short-lived changes in climate change beliefs during major heatwaves are likely to be explained by a salience effect rather than through a Bayesian process of updating (Bordalo et al., 2012; Deryugina, 2013; Larcom et al., 2019).

Based on this theoretical backdrop and the existing literature, we proceed to test the following hypotheses. Consistent with the theory of Bayesian Updating and the reviewed literature, we hypothesise (H1) that personal experiences of flooding and heatwave events increase risk perceptions (i.e., the perceived likelihood of similar and related future events) and climate change concern. However, we do not expect this to be the case for pro-environmental behaviour, as previous literature has found no effect (Larcom et al., 2019). To explore the average treatment effects of exposure, we define "personal experience" following standard approaches implemented in the literature (detailed below). Despite having clear expectations regarding the direction of the treatment effects, we proceed conservatively by reporting two-sided significance levels throughout the analysis.

[6] However, it is important to note that people's goals or motivations may affect the belief updating process. For instance individuals may engage in directional 'motivated reasoning', by which new evidence is interpreted in such that it maintains one's prior beliefs (Bayes & Druckman, 2021; Druckman & Mcgrath, 2019).

[7] Brügger et al. (2021) review the broader psychological literature and formulate a range of testable hypotheses about when and how experiences are likely to trigger different types of cognitive, emotional, and behavioural responses.

While average treatment effects provide a basis for comparison with the previous literature, our data allows us to explore more nuanced effects of extreme weather events on climate change attitudes. Thus, we formulate testable hypotheses for the following three dimensions: (1) Treatment Intensity, (2) Temporal Proximity and (3) Event Frequency.

First, we explore differences in treatment intensity for both flooding and heatwaves. For flooding, we hypothesise (H2) that closer proximity to flood events is associated with larger increases in risk perceptions and climate change concern. The closer the event occurs to the household, the more personally relevant and consequential its impacts might be (Brügger et al., 2021). In contrast, we expect flood proximity to be negatively associated with pro-environmental behaviour (H3). Individuals who have directly suffered negative impacts (emotional or financial) may be reluctant to adopt effortful behaviours (Brügger et al., 2015). The same line of reasoning argues that intense emotional experiences may either mediate increased concern and action on climate change (Demski et al., 2017), or motivate people to deny and distance themselves from climate change to reduce unpleasant emotions such as anxiety or fear (Hamilton-Webb et al., 2017; McDonald et al., 2015). We explore this hypothesis (H4) using measures of subjective well-being from the UKHLS survey. For heatwaves, we hypothesise (H5) that longer heatwave duration is associated with larger changes in climate change beliefs. Longer heatwaves are likely to be perceived as more unusual and hence be more salient than shorter heatwave spells. Moreover, longer heatwave spells may compromise physical well-being, especially for the elderly. In sum, we would assume that longer periods of heat wave exposure lead to a larger change in attitudes towards climate change, consistent with a Bayesian process of belief updating (Deryugina, 2013).

Second, we explore the role of temporal proximity to the event. Consistent with a 'salience effect' we test the hypothesis (H6) that the effect of flooding and heatwave exposure diminishes the greater the time between the event and the UKHLS interview date. Moreover, we explore whether the relationship between event proximity and climate change attitudes is linear or non-linear.

Third, we focus on event frequency, motivated by mounting evidence that increasing frequency of extreme weather events in the UK is being caused by climate change (Christidis et al., 2015; Kendon et al., 2014). Several potential psychological processes may underlie the relationship between event frequency and climate change attitudes. First, the more frequent a certain event, the more likely people are to be personally affected by it and hence notice and remember it. Furthermore, more frequent events may be perceived as more unusual and attribution to climate change may be facilitated by media coverage on the abnormality of recurring events.

We, thus, hypothesise (H7) that greater frequency of flooding and heatwaves is associated with larger changes in risk perceptions, climate change concern and pro-environmental behaviours.

2.3 Data description and empirical approach

2.3.1 Data

Climate change perceptions and pro-environmental behaviour

Data on climate change attitudes and environment related behaviour come from the UK Household Longitudinal Study (UKHLS). The UKHLS is a large annual household panel survey that follows the lives of approximately 40,000 households in yearly intervals since 2009. A feature of our analysis that differentiates it from other work mentioned in Section 2.2 is that we were granted access to the 'secure access' version of the UKHLS dataset (University of Essex, 2020), which provides geo-referenced location information for each household. Households are assigned a grid reference (a location to the nearest metre) based on their postcode at the time of the UKHLS interview.

The first, fourth and tenth waves included an additional environmental behaviour questionnaire module which contains a rich set of questions on climate change attitudes, risk perception, as well as individual environmental behaviours. Our empirical strategy does not arbitrarily select some of these questions, but instead utilises all the wealth of information contained in the data. We explore multiple dimensions of climate change attitudes by constructing three dependent variables. First, we measure climate change risk perception based on responses to the question: *Do you believe that people in the UK will be affected by climate change in the next 30 years.* Second, we construct an index for climate change concern based on responses to nine questions around environmental and climate change attitudes (e.g., *I don't believe my behaviour and everyday lifestyle contribute to climate change*). As all nine variables are highly correlated, we conducted a factor analysis to predict an underlying 'Climate Concern Factor' for each individual.[8] Finally, we construct an index of environmental behaviour based on self-reported environmental habits including household, consumption and travel behaviours. Respondents indicated how frequently they engaged in each behaviour, ranging from 'never' (1) to 'always' (5), or "not applicable". Variables were recoded so that positive values reflect more pro-environmental behaviour, and the index was calculated by

[8]The response format to the environmental attitude questions was changed from a binary response format in Wave 1 to a 5-point Likert Scale in Waves 4 and 10. To ensure consistency in responses, we restrict our data to a two-period panel of Waves 4 and 10 when exploring changes in climate change concern.

taking the sample average frequency for all behaviours applicable to the individual respondent. An overview of all questions that are used to construct the dependent variables for our empirical analysis are presented in Appendix Table 2.A1.

Flooding and heatwave data

Flooding data for England and Wales comes from the 'Recorded Flood Outlines' database maintained by the UK Environment Agency[9] and the 'Recorded Flood Extents' dataset published by Natural Resources Wales.[10] Both geospatial datasets contain all records of historic flooding from rivers, the sea, ground and surface water for England and Wales, respectively, providing detailed information on each event, as well as their exact geographic extent. For our matching strategy (discussed in Section 2.3.3), we further utilise geospatial flood vulnerability indicators available via the Climate Just Tool (Sayers et al., 2017) and the Environment Agency's 'Risk of Flooding from Rivers and Seas' (RoFRAS).[11] Temperature data was obtained from the 'HadUK-Grid' dataset maintained by the Met Office and made available via the Centre of Environmental Data Analysis (Hollis et al., 2019). The dataset contains gridded climate variables, interpolated from meteorological station data, at a resolution of 1 x 1 km for the entirety of the UK. To construct heatwave indicators for our analysis, we extracted daily maximum temperature records at the exact household location of each UKHLS participant at the time of the UKHLS interview.

2.3.2 Treatment assignment

We use GIS software to identify individual level flood and heatwave exposure by linking the exact household location of each UKHLS participant recorded on the date of the interview with spatial flood and temperature maps. An individual is allocated to the flood treatment group if at least one flood (as defined and recorded by the official sources mentioned above) occurred within a 1,000-metre radius during the observation period (2009-2020). The data were mapped and spatially joined using QGIS3.16.0. The spatial-join procedure is displayed in Figure 2.3.1.

[9] Downloadable from: https://data.gov.uk/dataset/16e32c53-35a6-4d54-a111-ca09031eaaaf/recorded-flood-outlines

[10] Downloadable from: https://lle.gov.wales/catalogue/item/HistoricFl/?lang=en

[11] Downloadable from: https://data.gov.uk/dataset/bad20199-6d39-4aad-8564-26a46778fd94/risk-of-flooding-from-rivers-and-sea

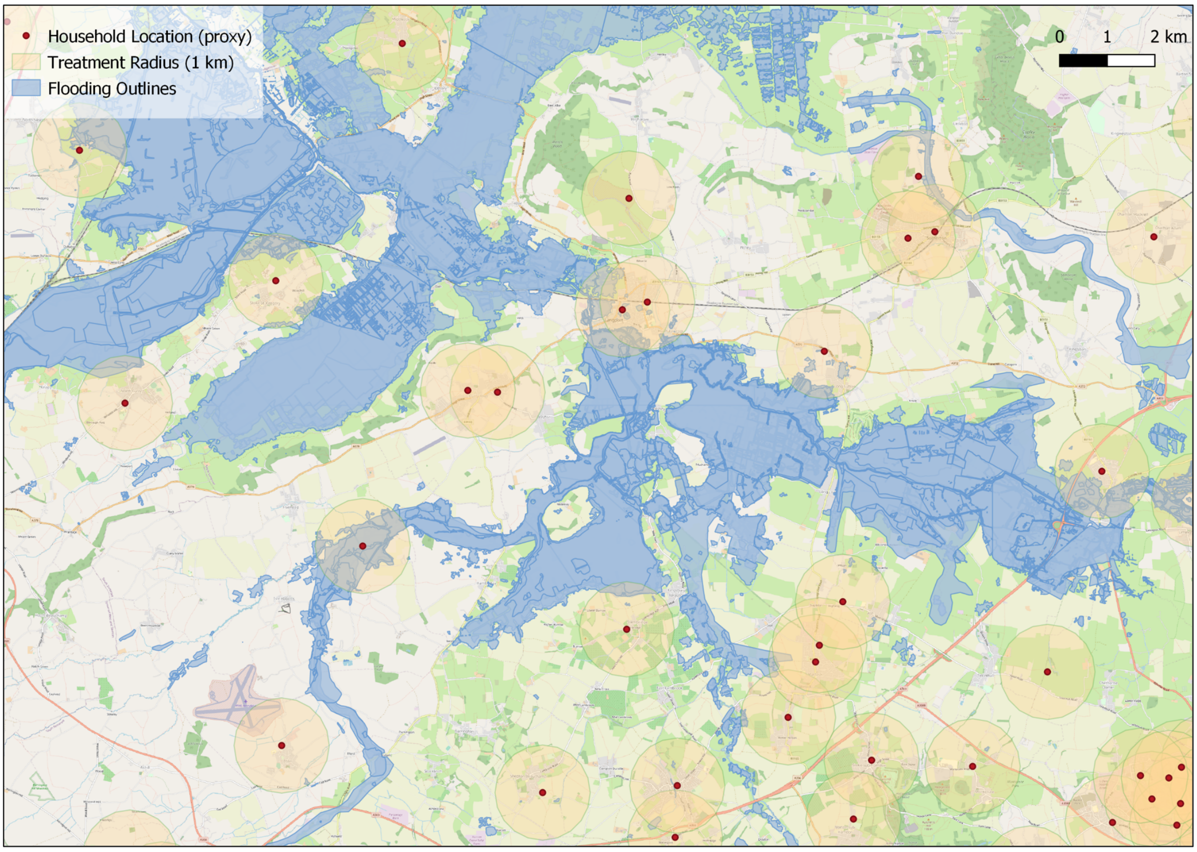

Figure 2.3.1: Treatment assignment flooding – GIS spatial-join procedure

If a flood outline intersects the 1-km radius surrounding each household location, then individuals of this household are assigned to the treatment group.[12] Additionally, we calculate the smallest distance between the household location and the flood outline using the "Join attributes by nearest" tool. This spatial-join procedure is repeated for each flood outline within the vicinity of the household (e.g., floods that occurred in the same area but in different years).

We utilise all flood outlines recorded between 2007 and 2020 identified by a unique flood event code and their start and end date. For our main analysis, we consider a 1,000-metre radius as our primary treatment definition. In a supplementary analysis we expand the treatment radius to 2,000 meters to explore sensitivity to proximity to the flood event. We do so by interacting a binary treatment indicator for flood experience within a 2,000-metre radius with a continuous variable for the minimum experienced distance to the flood event. For both

[12]Please note that Figure 2.3.1 displays proxy household locations and no sensitive information is disclosed.

radii, we exclude individuals who had already experienced a flood event within a two-years prior to their first interview, as their treatment status provides no time-varying information for our within-individual analysis.[13] Moreover, by excluding individuals with prior flood exposure, the focus of our analysis lies on individuals for whom floods are particularly novel and distinctive events (i.e. unusual and noticeable) (Brügger et al., 2021). The degree of abnormality or unexpectedness has been found to be a significant predictor of attention to climate-related events (Sisco et al., 2017). [14]

Figure 2.3.2 displays the GIS treatment assignment procedure for heatwave exposure. The procedure involves mapping the temperature grids at a resolution of 1 x 1 km for the entirety of the UK (left panel) and overlaying the exact household locations of the UKHLS participants (right panel). We use the "Sample Raster Values" tool in QGIS3.16.0 to extract daily minimum and maximum temperature values at each household location.

[13]While the majority of individuals were interviewed in Wave 1 (2009-2010), 15.50% of individuals only joined the panel in Wave 4 (2013-2014). We thus specify a flexible two-year time window to identify prior-flood experience based on the first recorded interview date specific to each individual. As we do not have complete information on individuals' geographic location prior to the study period, setting a two-year window minimises the likelihood that someone is falsely identified with prior flood experience, but provides a sufficient time-period to determine prior exposure.

[14]We argue that once an individual has experienced a flood, they should not "switch back" to the control group if the flood event lies outside a specific window from the next survey date. Hence, in our analysis, an individual remains in the treated group after they have experienced an event.

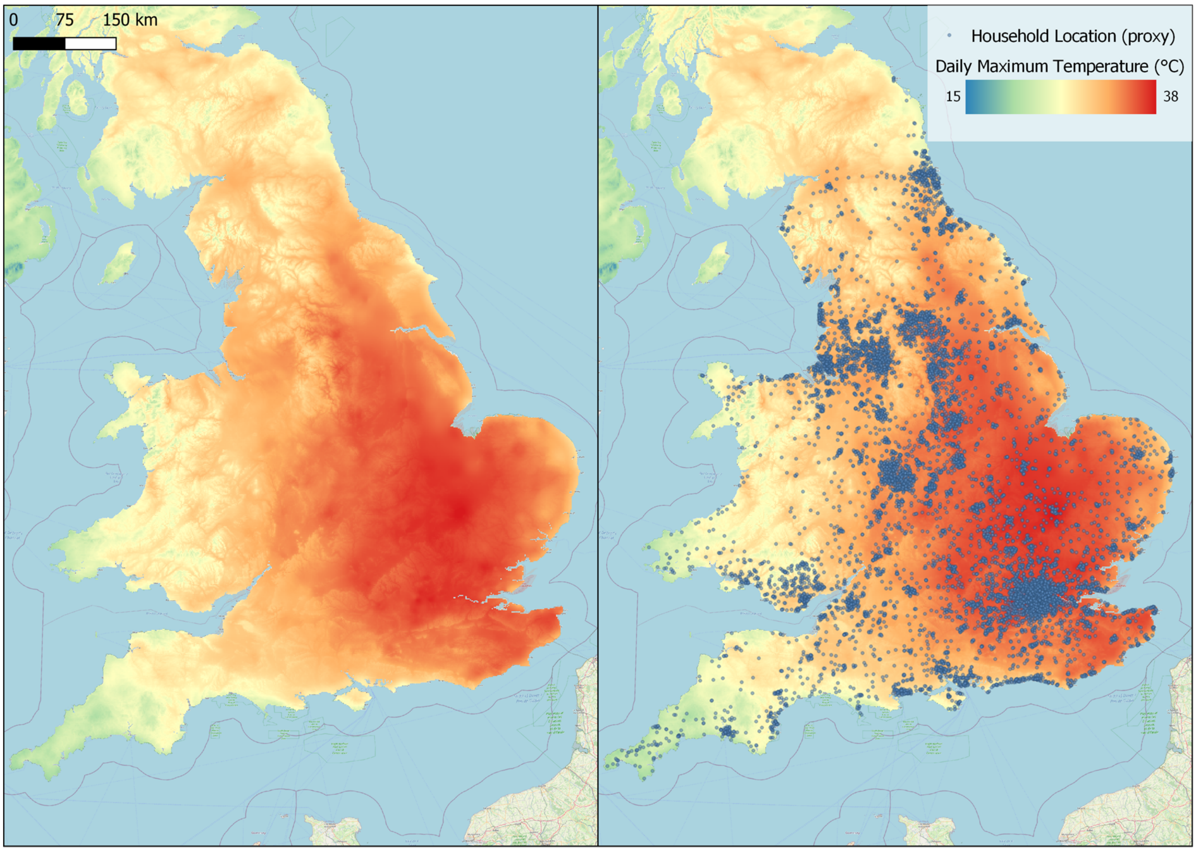

Figure 2.3.2: Treatment assignment heatwaves – GIS spatial-join procedure

Note: Sample temperature gird of maximum temperatures recorded on 25th July 2019.

We define heatwave exposure as having experienced at least three consecutive days with day-time maximum temperatures exceeding 29°C. While no commonly accepted definition of a heatwave in the UK exists, our definition of heatwave experience has been applied in previous empirical research (Larcom et al., 2019). The World Meteorological Organisation defines heat waves as "unusual hot weather (Max, Min and daily average) over a region persisting at least two consecutive days during the hot period of the year based on local climatological conditions, with thermal conditions recorded above given thresholds."(WMO, 2018). The definition suggests that heat waves are characterised by their magnitude (temperature or anomaly threshold surpassed), their duration (consecutive days) and their extent (geographical area affected). To explore the nuances of heatwave intensity, we construct an additional measure of heatwave duration, which counts the numbers of consecutive heatwave days experienced.

2.3.3 Identification

To identify the causal effect of flood and heatwave experience on our three dependent variables, we rely on the assumption of parallel trends, which implies that the treatment and control groups would follow common trends in outcomes in the absence of treatment. In the absence of additional pre-treatment periods, we are unable to perform standard tests exploring the equality of trends prior to flood and heatwave exposure. Moreover, residential sorting over flood risk poses a challenge for empirical identification (Bakkensen & Ma, 2020). Nevertheless, we can take several precautionary ex-ante measures to strengthen the plausibility of the parallel trends assumption in our data. In the following sections, we address several factors which may threaten the validity of our identification strategy and discuss our propensity score matching approach to mitigate concerns about diverging trends between treatment and control group.

Residential sorting

A major challenge to empirical identification is residential sorting. Residential sorting happens when individuals self-select into or away from areas which are more likely to experience extreme events. If residential sorting is endogenous to event experience, the effect on climate change attitudes is likely to be biased. A first concern relates to residential sorting that occurred during the observation period. For instance, experiencing an extreme event may induce people to move to a different area. To mitigate the threat from residential sorting that occurred during the observation period, we exclude all residents (both treatment and control units) that moved during the observation period. [15]

A second concern relates to residential sorting that occurred prior to the observation period, which is not directly observable. The fact that treatment is not exogenously allocated across space and time may invalidate the assumption that allocation to treatment is independent of potential outcomes. For instance, flooding is much more likely to be experienced by households living near rivers and sea and especially likely for properties constructed on flood plains. If people sort over flood risk, it could be argued that those living in areas more susceptible to flooding are systematically different from people living elsewhere. In support

[15] We take a conservative approach by excluding all individuals who moved more than 1000 meters from their initial household location at any time during the observation period. Based on this definition of a mover, 11.26% of the sample moved at some point during the observation period. Identifying the exact household location is particularly important in the case of flood exposure, which may warrant an even more conservative approach. However, we argue that households that move less than a kilometre away remain within the same community and thus would "experience" flooding in a similar way. Nonetheless, our main results are robust to alternative definitions of movers (i.e., people moving at least 100 meters, 500 meters or outside of their LSOA).

of this argument, recent research shows that flood vulnerability is associated with a range of socio-spatial factors (Sayers et al., 2017). In turn, people willing to live in flood-prone areas may be more risk-loving than people who choose to live in safe distance to flood plains. On the other hand, exposure to heatwaves is likely to also be associated with a range of socio-spatial factors. Heatwaves are much more likely to occur in southern regions of the UK and may be especially severe in cities (reinforced by the urban heat island effect) and more moderate close to the coast. While systematic differences in levels between treated and control units pose no immediate threat to the internal validity of our analysis, we may still be concerned that differences may be associated with diverging trends in outcomes for the two groups, which would violate our key identifying assumption. To mitigate this concern, we take several actions. First, we utilise a generalised DID approach with individual fixed effects which account for any individual differences which are constant over time. Second, we employ a propensity score matching approach to identify a subset of control units prior to analysis, which are more comparable to the treatment group. The details of our matching strategy are discussed next.

Matching strategy

In our case, we use Propensity Score Matching (PSM) to select a set of individuals from the control group who are 'comparable' to the treatment group, based on observable characteristics. The reasoning goes as follows: by selecting a control group with PSM we minimise any potential bias that may arise from selection into treatment. In the absence of pre-treatment data, we are unable to test for common trends in pre-treatment outcomes between treatment and control groups. However, a key advantage of the nearest-neighbour matching approach is that it narrows down the control group to units which are observationally similar to treated units and thus more likely to follow similar trends (Deryugina et al., 2020).

We construct the matching criteria for our primary definitions of flooding and heatwave exposure using data from multiple sources: First, we use a selection of small-area statistics drawn from the 2011 Census at the Lower Layer Super Output Area (LSOA) level,[16] including population density, unemployment rate, share of income deprived households and the share elderly people aged over 75. The small-area statistics provide important socio-economic background information at the macro-level which are predictive of both flooding and heatwave exposure. Second, we use individual baseline characteristics from the UKHLS dataset, including net annual household income, highest attained qualification (education) and hous-

[16]LSOAs are geographic areas designed for reporting of small area statistics with an average area of 4 km^2 and a mean population of 1500.

ing tenure. As our panel data is unbalanced, we include an additional matching variable which captures in which wave an individual completed the UKHLS questionnaire. This allows us to incorporate how many years and in which years each individual participated in the survey into the matching process. Including individual-level characteristics into the matching equation allows us to identify comparable control units with greater precision. Finally, we include a set of regional dummy variables and a rural/urban indicator.

To identify the propensity of flood exposure, we use additional flood-specific variables. To capture direct flood exposure at the macro (LSOA) level, we use information on the share of properties exposed to significant flood risk. To obtain an even more precise estimate of household-level flood risk, we utilise the 'Risk of Flooding from Rivers and Seas' (ROFRAS) dataset for England and Wales which provides a spatial representation of flood risk and classifies areas into very low, low, medium and high-risk areas. Using GIS, we identify the flood-risk of each UKHLS respondent based on a 500-metre radius. Finally, we include an index of neighbourhood flood vulnerability from the 'Climate Just Online Tool' to capture socio-spatial vulnerability (Sayers et al., 2017). The index captures neighbourhood flood vulnerability based on a pre-defined set of vulnerability criteria measured at the LSOA level.

Our matching approach follows a standard two-stage procedure (Imbens et al., 2009; Leuven & Sianesi, 2003). We first predict the propensity score of being exposed to an extreme event using the variables outlined above in a probit model, where the dependent variable is an indicator for treatment assignment (See Section 2.3.2). We repeat this procedure separately for each flooding treatment radius as well as heatwave experience, excluding the flood-specific vulnerability and risk indicators for the latter. Subsequently, we use the estimated propensity score to identify the three nearest neighbours for each treated individual from the individuals that were not treated as per our treatment definitions.[17] Using this approach, we create a matched sample for our analysis and estimate the Average Treatment Effect on the Treated using a generalised difference-in-differences specification.

[17]Our main results are robust to matching treated units to their two and five nearest neighbours, respectively. However, matching on three nearest neighbours performs best in balancing treatment and control groups on observable characteristics and baseline preferences. Moreover, an important assumption for PSM is that there is sufficient common support across treatment and control group covariates to create reasonable propensity score matches. In the case of heatwave exposure, common support is not achieved for a subset of treatment units. We choose to drop these treated observations from our matched sample to achieve greater balance between treatment and control groups. In total, we drop 866 treated units not on common support or for which matching variables were missing. Retaining these individuals in our analysis does not substantially change the results, however, balance is greatly improved when these observations are not included.

2.3.4 Summary statistics

Table 2.3.1 shows the number of untreated and treated individuals based on our primary definitions of flood and heatwave exposure. Numbers displayed in columns (1) and (2) are obtained from the full sample, after excluding movers (as defined above) and those individuals who did not complete the climate attitudes questionnaire, which we used to construct the outcome variables. Columns (3) and (4) display the untreated and treated units which are retained after completing the matching procedure. As previously discussed, a small number of individuals are dropped from each treatment group due to the unavailability of high-quality matches (i.e., not on common support) or missing matching variables.

Table 2.3.1: Treated and untreated samples

	Full Sample		Matched Sample	
	(1) Untreated	(2) Treated	(3) Untreated	(4) Treated
Flood Exposure (1,000 m)	23,588	1,915	3,601	1,895
Flood Exposure (2,000 m)	20,944	3,460	5,314	3,436
Heatwave Exposure	18,967	7,450	4,556	6,584

Note: Flood Exposure is defined as living within a 1000(2000)-meter radius from a recorded flood extent, respectively. Heatwave Exposure is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29°C.

Tables 2.3.2 presents summary statistics for the full (unmatched) and matched samples based on our primary treatment definition for flood exposure. Panel A shows individual socio-demographic characteristics measured at baseline, Panel B presents the pre-treatment outcome variables, and Panel C shows the exposure (treatment) variable. Columns (1), (2), (4) and (5) present the means for control and treatment groups for the unmatched and matched samples, respectively. Columns (3) and (7) show the differences in means and corresponding p-values obtained from a two-sided comparison of means between treatment and control group. The comparison of means between unmatched and matched samples further illustrates the benefits of the nearest-neighbour matching approach. In the unmatched sample we find statistically significant differences in education, tenure status, education level, age and health. However, after selecting a more comparable sub-sample of control units for the matched sample, only the difference in health status and tenure status between treatment and control groups remain statistically significant at a 5% level (column 6), suggesting that the matching procedure improved the balance on socio-demographic and pre-treatment preferences between the two groups.

Table 2.3.2: Summary statistics: Flood exposure

	Full Sample				Matched Sample			
	(1) Untreated	(2) Treated	(3) Difference	(4) Obs.	(5) Untreated	(6) Treated	(7) Difference	(8) Obs.
Outcome Variables								
Risk Perception (Yes = 1)	0.748	0.752	-0.004	20486	0.756	0.750	0.005	4579
Concern Index	-0.179	-0.143	-0.036	20750	-0.160	-0.145	-0.015	4662
Behaviour Index	2.997	2.997	0.000	24626	2.985	2.997	-0.012	5287
Socio-demographic Indicators								
Household Income (£)	2784.334	2813.477	-29.144	24626	2822.230	2804.876	17.355	5287
Education Level	3.593	3.820	-0.226***	24580	3.791	3.812	-0.022	5287
House Owned (Yes = 1)	0.745	0.826	-0.081***	24554	0.804	0.826	-0.022**	5287
Age (Years)	48.558	49.404	-0.846**	24626	49.580	49.479	0.101	5287
Chronic Health Condition	0.372	0.345	0.028**	24614	0.373	0.345	0.028**	5285
Rural (Yes = 1)	0.211	0.214	-0.003	24626	0.224	0.212	0.012	5287

Note: Table displays the mean values of baseline outcome variables and socio-demographic characteristics, for untreated and treated groups based on our primary definition of flood exposure (1000m) in the full and matched samples. Columns (3) and (7) report the difference between the mean values measured at baseline between untreated and treated groups and signficance stars correspond to p-values obtained from a two-sided t-test for comparison of means. Obs. refers to the number of observations used to compute the means and conduct the t-test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3.3 reports the summary statistics for the unmatched and matched samples based on our definition of heatwave exposure.The results of the two-sided t-test in Column (3), displayed as significance stars, imply that there are significant differences between treatment and control groups for nearly all socio-demographic characteristics and baseline climate change attitudes in the unmatched sample. Treated individuals have higher average income, education, are slightly younger, less likely to suffer from a chronic health condition and less likely to live in rural areas. Moreover, the treated group has a higher baseline level of climate change risk perception, concern and pro-environmental behaviour. While nearest neighbour matching reduces the differences in means between treatment and control group, it is unable to completely eliminate statistically significant differences in all socio-demographic characteristics. More importantly however, our matching approach greatly improves balance on pre-treatment outcomes and differences between the two groups for these variables are no longer statistically significant.

Table 2.3.3: Summary statistics: Heatwave exposure

	Full Sample				Matched Sample			
	(1) Untreated	(2) Treated	(3) Difference	(4) Obs.	(5) Untreated	(6) Treated	(7) Difference	(8) Obs.
Outcome Variables								
Risk Perception (Yes = 1)	0.745	0.762	-0.017**	21280	0.772	0.760	0.013	9087
Concern Index	-0.211	-0.090	-0.121***	21557	-0.126	-0.100	-0.026	9244
Behaviour Index	2.980	3.045	-0.066***	25544	3.026	3.034	-0.007	10767
Socio-demographic Indicators								
Household Income (£)	2656.307	3083.761	-427.454***	25544	2889.109	3010.095	-120.986***	10767
Education Level	3.501	3.896	-0.395***	25497	3.692	3.847	-0.155***	10767
House Owned (Yes = 1)	0.750	0.755	-0.005	25466	0.756	0.766	-0.009	10767
Age (Years)	49.008	47.952	1.056***	25544	48.586	48.457	0.129	10767
Chronic Health Condition	0.385	0.336	0.049***	25531	0.364	0.344	0.021**	10761
Rural (Yes = 1)	0.223	0.179	0.044***	25544	0.239	0.201	0.039***	10767

Note: Table displays the mean values of baseline outcome variables and socio-demographic characteristics, for untreated and treated groups based on our primary definition of heatwave exposure (1000m) in the full and matched samples. Columns (3) and (7) report the difference between the mean values measured at baseline between untreated and treated groups and signficance stars correspond to p-values obtained from a two-sided t-test for comparison of means. Obs. refers to the number of observations used to compute the means and conduct the t-test.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.3.5 Empirical strategy

To estimate the effect of extreme weather events on climate change beliefs and pro-environmental behaviour we utilise a two-way fixed effects (TWFE) generalised difference-in-differences (DD) model. Our baseline model, following Baker et al. (2022) can be expressed as follows:

$$Y_{it} = \alpha_i + \lambda_t + \delta^{DD} Event_{it} + \varepsilon_{it} \tag{2.1}$$

where Y_{it} is the measure of risk perception, climate change concern or pro-environmental behaviour of individual i at time t. Individual fixed effects are captured by α_i, which account for any unobserved time-invariant individual characteristics. λ_t are survey-wave-by-season fixed effects which account for common changes in climate-change beliefs over time specific to the season in which the UKHLS questionnaire was completed. ε_{it} is the random error term. Our baseline specification does not include any additional time-varying control variables.

$Event_{it} = Treat \times Post$ is an indicator for a treated individual ($Treat$) after a flood or heatwave event occurred within their vicinity ($Post$), with both main effects being subsumed by the unit and time fixed effects (Baker et al., 2022). In our baseline specification, flood treatment is defined as living within a 1000-metre radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29°C. The coefficient of interest is δ^{DD} which represents the difference-in-differences estimator.[18] We estimate the average treatment effect on the treated (ATT) from equation (2.1) by OLS for each outcome variable (Y_{it}) separately. As a robustness check, we estimate equation (2.1) including a vector X_{it} of time-varying socio-economic control variables which have been identified as important predictors of climate change attitudes. These include income, education and housing tenure.

We then estimate three alternative specifications to explore the effect of treatment intensity and temporal proximity, following our hypotheses laid out in Section 2.2.2. First, we allow the intensity of treatment to vary, as specified in equation (2.2). $Intensity_{it}$ serves as a placeholder for various measures of treatment intensity: We initially allow the treatment effect to vary by distance to the flood event. We estimate the distance effect with a continuous variable ($MinDis_{it}$) which captures the minimum recorded distance to the flood event for treated individuals. For heatwaves, we are interested in whether treatment intensity is associated with heatwave duration. We construct a continuous measure ($MaxDur_{it}$) for the maximum number of consecutive days experienced during a heatwave episode. As we have no a priori assumptions of whether the treatment intensity has a linear or non-linear effect on climate change attitudes, we estimate both linear and quadratic functions of treatment intensity $f(Intensity_{it})$, the latter shown in equation (2.2):

$$Y_{it} = \alpha_i + \lambda_t + \delta^{DD} Event_{it} + \theta(Event_{it} \times f(Intensity_{it})) + \varepsilon_{it} \quad (2.2)$$

$$Y_{it} = \alpha_i + \lambda_t + \delta^{DD} Event_{it} + \theta(Event_{it} \times f(Months_{it})) + \varepsilon_{it} \quad (2.3)$$

$$Y_{it} = \alpha_i + \lambda_t + \delta^{DD} Event_{it} + \theta(Event_{it} \times f(Frequency_{it})) + \varepsilon_{it} \quad (2.4)$$

[18] A recent literature shows the potential bias arising in generalised DID designs with staggered treatment timing (Baker et al., 2022; Goodman-Bacon, 2021). As a robustness check, we estimate the efficient estimator proposed by Callaway and Sant'Anna (2021).

Second, we explore temporal proximity by allowing treatment impact to vary by the number of months ($fMonths_{it}$) since the event occurred. We assume non-linearity in the treatment effect over time and model this relationship using a quadratic function $f(Months_{it})$. Finally, we explore the effect of event frequency using a continuous variable capturing the cumulative number of events experienced by individual i at time t. To provide easily interpretable results, we estimate and plot the marginal treatment effects at representative values of treatment intensity , months since the event and event frequency.

While our empirical strategy allows a causal interpretation of the results, it is important to note that our identification strategy follows an 'intention to treat' approach. Respondents with flood and heatwave exposure were identified based on objective measures of flood and heatwave incidence alone. The actual individual subjective experience of the events remains unknown, and we are unable to ascertain that the respondents were physically present at the time of the weather event. Nonetheless, we argue that the household location is a good proxy for flood experience, whether direct (physically present at the time of the weather event) or indirect (via affected friends and family members). Furthermore, the use of objective GIS data avoids potential biases commonly encountered with subjective measures of flood experience (Guiteras et al., 2015). For instance, self-reports of flood experience and damages may be subject to recall bias or influenced by other unobserved individual-level characteristics (Hassan, 2006).

2.4 Results

2.4.1 Average treatment effects

The following section we present the average treatment effects obtained from our analysis of equation (2.1) following our primary definitions of flooding and heatwave exposure. Flood exposure is primarily defined as living within a 1000-metre radius from a flood and heatwave exposure is defined as having experienced at least three consecutive days of maximum temperatures above 29°Celsius. Figure 2.4.1 provides a visual representation of the ATT of event experience on our three outcome variables. The full results are shown in Appendix Table 2.A2.

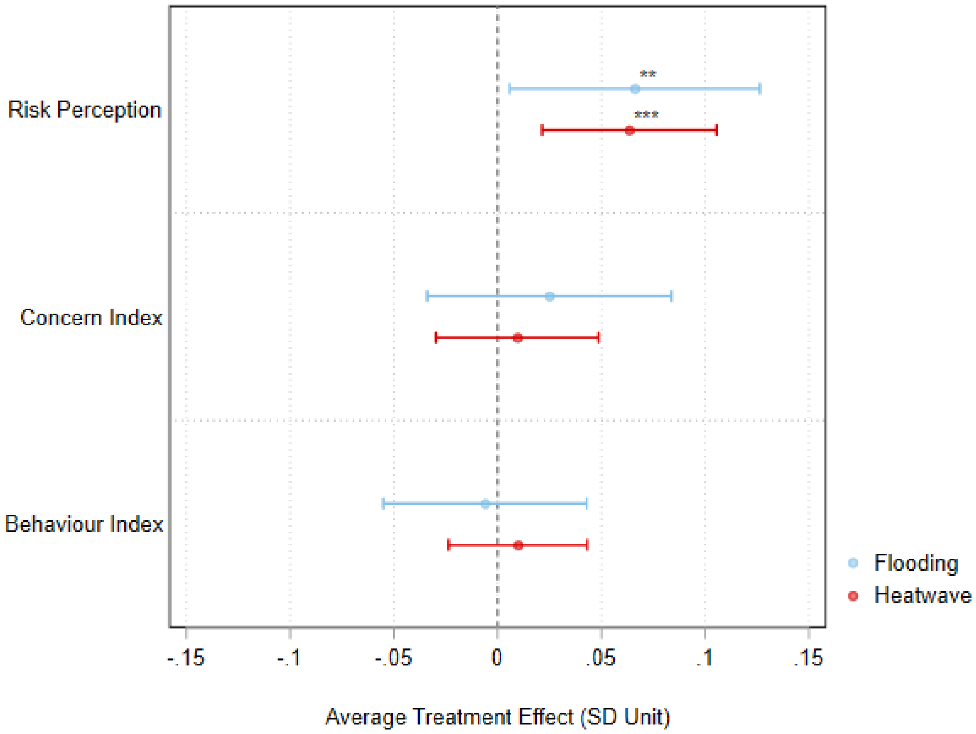

Figure 2.4.1: Average treatment effects of flooding and heatwave exposure

Note: OLS estimates of equation (2.1). Error bars indicate 95% confidence intervals. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results indicate that both flood and heatwave experience increase climate change risk perception but have no statistically significant average effect on climate change concern and pro-environmental behaviour. Flood experience leads to an increase in risk perception by 0.066) standard deviation units (or 2.2 percentage points), significant at the 5% level. Heatwave experience causes a more precisely estimated increase of the same magnitude (0.063 SD units), significant at the 1% level. The estimates for climate change concern and pro-environmental behaviour are all close to zero and statistically indistinguishable from zero.[19]

The findings are only partially in line with our hypothesis that extreme event experience increases risk perceptions *and* concern. However, the positive result that does emerge from our analysis confirms the risk perceptions hypothesis formulated in Brügger et al. (2021) that

[19]The main results are robust if estimated using the efficient estimator proposed by Callaway and Sant'Anna (2021). See Appendix Table 2.A3. Therefore, we conclude that the staggered treatment setting is not a significant source of bias in our setting and proceed using the standard TWFE estimator for the remainder of this chapter.

personal experience of climate related events should increase the perceived likelihood of similar and related events in the future. Our results indicate that after having experienced flooding or heatwaves, people are significantly more likely to believe that the UK will be affected by climate change in the next 30 years. As expected, extreme event exposure had no statistically significant impact on pro-environmental behaviour.

2.4.2 Treatment intensity

Next, we explore multiple measures of treatment intensity, which may reveal more nuanced effects of extreme event exposure. As discussed in Section 2.2.2, we hypothesised that greater proximity to flood events is associated with a larger increase in risk perceptions. The closer the event occurs to the household, the more personally relevant and consequential it is likely to be (Brügger et al., 2021). In the case of flooding, individuals may suffer direct damage to property or may be otherwise affected from damage to infrastructure and services. Moreover, a growing literature has documented the negative impacts flood experience can have on mental health and subjective well-being of flood victims (Hudson et al., 2017; Luechinger & Raschky, 2009; Milojevic et al., 2017).

To explore the effect of physical proximity to flooding, we construct a continuous variable capturing the minimum distance from the flood extent outline to the household location of people that experienced a flood within a radius of 2,000 metres. We allow the treatment radius to span 2,000 metres to evaluate whether a distance gradient exists at greater distances. Appendix Figure 2.A1 shows the distribution of minimum distances at which flood events were experienced in our sample. As with our primary treatment definition (1,000 metres), we identify a matched control group comparable to the treated units within a 2,000-metre radius using our nearest-neighbour matching strategy. Although we expect the effect of flood exposure to diminish with distance, we have no clear assumptions over the functional form and thus interact the treatment indicator with both the linear and quadratic term of distance to the flood event, respectively.

The results show that distance to the flood event is unrelated to climate change concern, but we find a clear linear relationship between proximity and risk perceptions (see Appendix Table 2.A4). Figure 2.4.2 visualises the relationship by plotting the DID estimates for flood exposure at distances between 0 and 2,000 metres. As changes in risk perceptions can be directly interpreted in percentage points, we plot the non-standardised estimates.

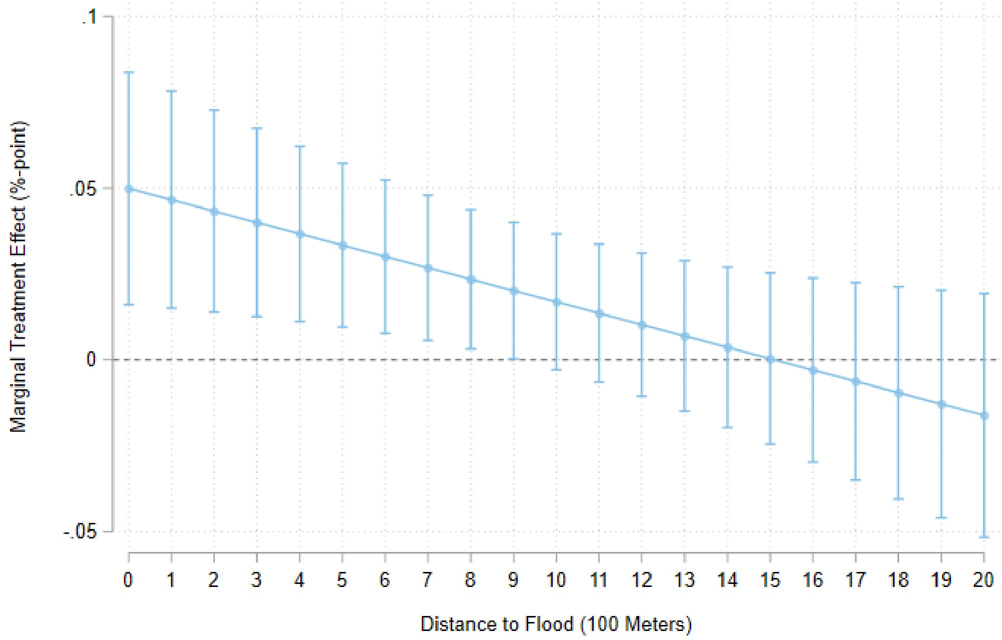

Figure 2.4.2: Marginal treatment effect (%-point change) of flood exposure with increasing distance to the flood

Note: Outcome: Risk Perception, $N = 18,712$. Error bars indicate 95% confidence intervals.

The effect of flood exposure on risk perceptions is largest for people living closest to the flood extent and diminishes with distance. At distances greater than 1,000 meters from the event, the effect is no-longer statistically different from zero. Our findings suggest that the effect of flooding is highly localised and confirm our hypothesis that the effect diminishes at greater distances. Contrarily, we find no such distance decay for climate change concern.

Next, we explore the effect of flood proximity on subjective well-being and pro-environmental behaviour. Previous evidence suggests that flood exposure may have an undesirable deterring impact on climate change engagement for those most adversely affected (Osberghaus & Demski, 2019). Moreover, negative emotions may give rise to a sense of helplessness and ack of ability to act on the matter, justifying in-action and a denial of responsibility as a form of coping-strategy (Brügger et al., 2015). Contrarily, emotionally charged events may also result in longer-lasting and more accurate memories, encouraging affected individuals to engage in risk minimisation if perceived self-efficacy is high (Brügger et al., 2021). The rich UKHLS dataset allows us to explore these hypotheses in more detail: We utilise self-reported life satisfaction as a measure of emotional response. Appendix Table 2.A4 shows that pro-environmental behaviour (columns 5 and 6) and self-reported satisfaction with life (columns 7 and 8) are both unrelated to flood exposure, regardless of the distance to the event. All estimates for both the linear and non-linear specification are close to zero and statistically insignificant.

Turning to heatwave treatment intensity, we hypothesised that the effect of heatwave experience may increase with the duration of the heatwave. We thus measure heatwave intensity as the maximum number of consecutive days with temperatures greater than 29°C experienced during the observation period. The distribution of heatwave duration in the treatment group is shown in Appendix Figure 2.A2. We estimate equation (2.2) to explore both linear and quadratic interactions of heatwave duration.

The results shown in Appendix Table 2.A5 indicate that heatwave duration appears to have a non-linear effect on pro-environmental behaviour but shows no statistically significant association with risk or concern. Three days of heatwave duration appear to have a negative impact, which increases and peaks at five days and subsequently diminishes. This relationship is visualised in Figure 2.4.3, however none of the individual point estimates are statistically different from zero.

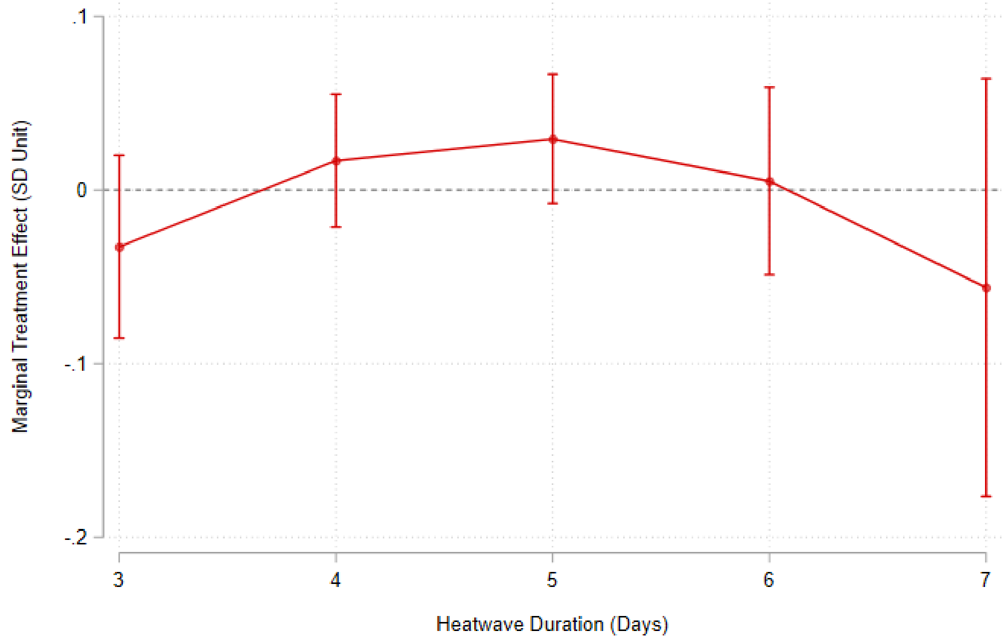

Figure 2.4.3: Marginal treatment effect (SD change) of heatwave exposure with increasing heatwave duration

Note: Outcome: Standardised Behaviour Index, $N = 26,935$. Error bars indicate 95% confidence intervals.

In sum, we observe that treatment intensity (measured as flood proximity and heatwave duration) is only somewhat associated with climate change attitudes. However, we find clear evidence that the effect of flooding on risk perceptions is highly localised, which provides support for the 'proximising' strategy, previously discussed.

2.4.3 Temporal proximity

In this section we explore the role of temporal proximity to the extreme event in shaping climate change attitudes. Based on salience theory and recent research (Larcom et al., 2019; Rüttenauer, 2021) we hypothesised that the effect of extreme events may diminish the greater the temporal distance between the event and the survey date. As specified in equation (2.3) we interact the treatment indicator with a continuous variable for the number of months passed since the event end date and estimate both a linear and quadratic specification. The distributions for both our primary definitions of flood and heatwave exposure are visually displayed in Figures 2.A3 and 2.A4.

Several interesting results emerge from this analysis. Full estimation results are presented in Appendix Tables 2.A6 and 2.A7. First, we find that the effect of flooding on risk perceptions diminishes the greater the temporal distance between the flood event and the survey date. This linear relationship is visualised in Figure 2.4.4 for a period of up to five years (60 month). The effect is no-longer statistically different from zero for individuals who were interviewed more than three years (36 months) after the flood event. Contrarily, we find no such relationship for heatwave exposure.

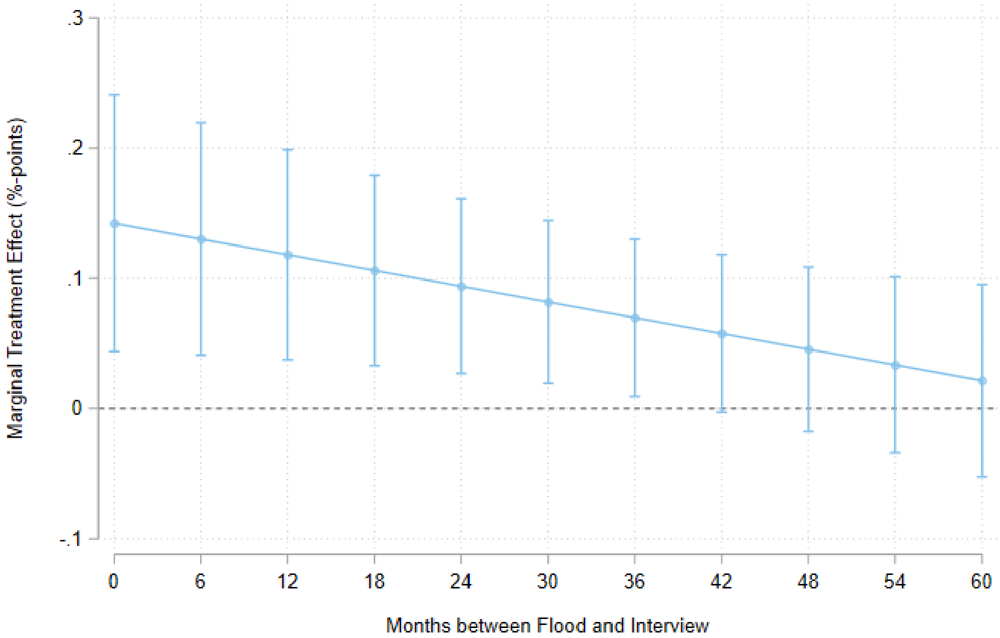

Figure 2.4.4: Marginal treatment effect (%-point change) of flood exposure with increasing number of months between the flood and the interview date

Note: Outcome: Risk Perception, $N = 11,990$. Error bars indicate 95% confidence intervals.

Second, we find a non-linear relationship between flood exposure and pro-environmental behaviour, shown in Figure 2.4.5. Flood exposure has a negative effect on stated pro-environmental behaviour for those interviewed within six months of the event. The negative effect then trends towards zero and turns positive after 18 months and peaks at 36 months (3 years). For individuals interviewed more than three years after the flood event, the effect diminishes and approaches zero. While this non-linear temporal relationship appears to be statistically significant at the 5% level, none of the individual DID estimates are statistically different from zero at conventional levels, suggesting that on average flood exposure has no effect on pro-environmental behaviour relative to a control group that did not experience flood events.

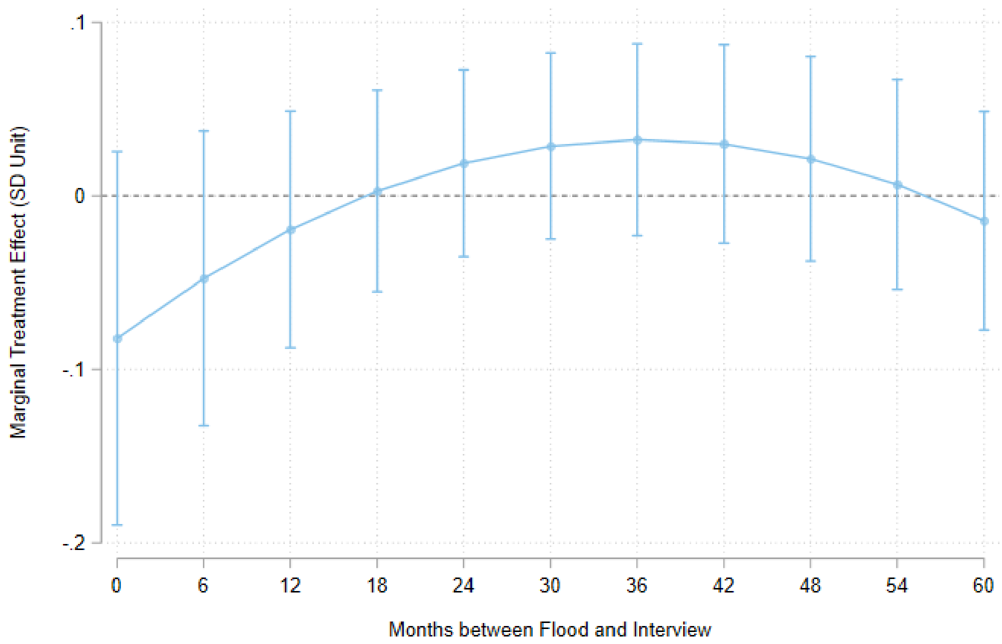

Figure 2.4.5: Marginal treatment effect (%-point change) of flood exposure with increasing number of months between the flood and the interview date

Note: Outcome: Standardised Behaviour Index, $N = 13,656$. Error bars indicate 95% confidence intervals.

Taken together, our analysis of temporal proximity finds only partial support for the premise that changes in climate change attitudes following extreme weather events are driven by a salience effect and are thus short-lived. In our data, this appears to be the case for flood exposure, but we find no such temporal-decay for heatwave exposure. Additionally, we find that the average treatment effect of flood exposure on pro-environmental behaviour may mask some important non-linear temporal effects. Flooding appears to have an initial negative effect on behaviour, which diminishes and turns positive over time.

2.4.4 Extreme event frequency

Next, we explore the hypothesis that more frequent events (i.e., multiple events within the observation period) have an incremental effect on climate change attitudes. The more frequent an event, the more salient and memorable it may be. Moreover, increasingly frequent events may be more readily attributable to anthropogenic warming. In the matched sample, the majority of respondents experienced a single flood or heatwave (78.31% for flooding, 51.25% for heatwaves). However, the remaining share of the sample was exposed to two or more events. Appendix Figures 2.A5 and 2.A6 show the distribution of the number of floods and heatwaves experienced by the respective treatment groups during the observation period. To avoid outliers biasing our results, those individuals who experienced more than three flood events (1.16%) are added to the category 3 (or more) floods. Nonetheless, the number of individuals who experienced more than one flood remains small, and hence the following results should be interpreted with caution. As previously discussed, we estimate both linear and quadratic models of equation (2.4) to explore the role of event frequency in shaping climate change attitudes.

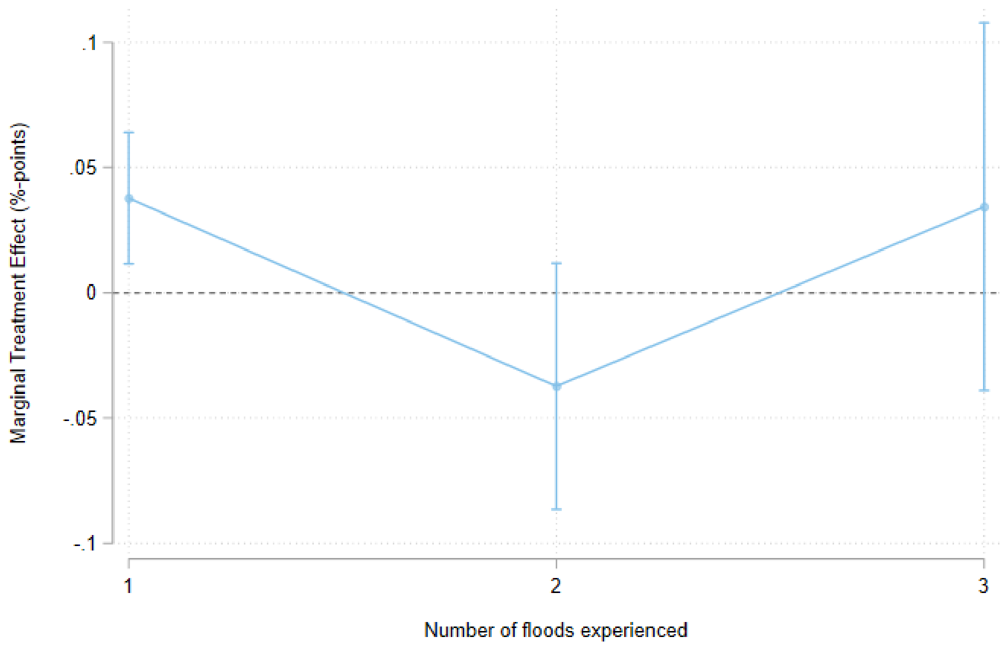

Figure 2.4.6: Marginal treatment effect (%-point change) of flood exposure with increasing number of floods experienced

Note: Outcome: Risk Perception, $N = 11,990$. Error bars indicate 95% confidence intervals.

The results for flooding are presented in Appendix Table 2.A8. Somewhat surprisingly, we find that experiencing multiple flood events decreases risk perceptions relative to those individuals who experienced only one event. This relationship is best explained by a quadratic function, which is visualised in Figure 2.4.6. The DID estimates for individuals exposed to two or three (or more) floods are no-longer statistically significant from zero. This unexpected finding may point to some form of adaptation behaviour for people who experienced multiple flood events, however, may also be a statistical artefact caused by the small sample size.

For heatwave exposure, the results are presented in Appendix Table 2.A9 and show that experiencing multiple heatwave events causes an increase in climate change concern and pro-environmental behaviour, significant at the 5% and 1% level, respectively. These linear relationships are visualised in Figure 2.4.7.

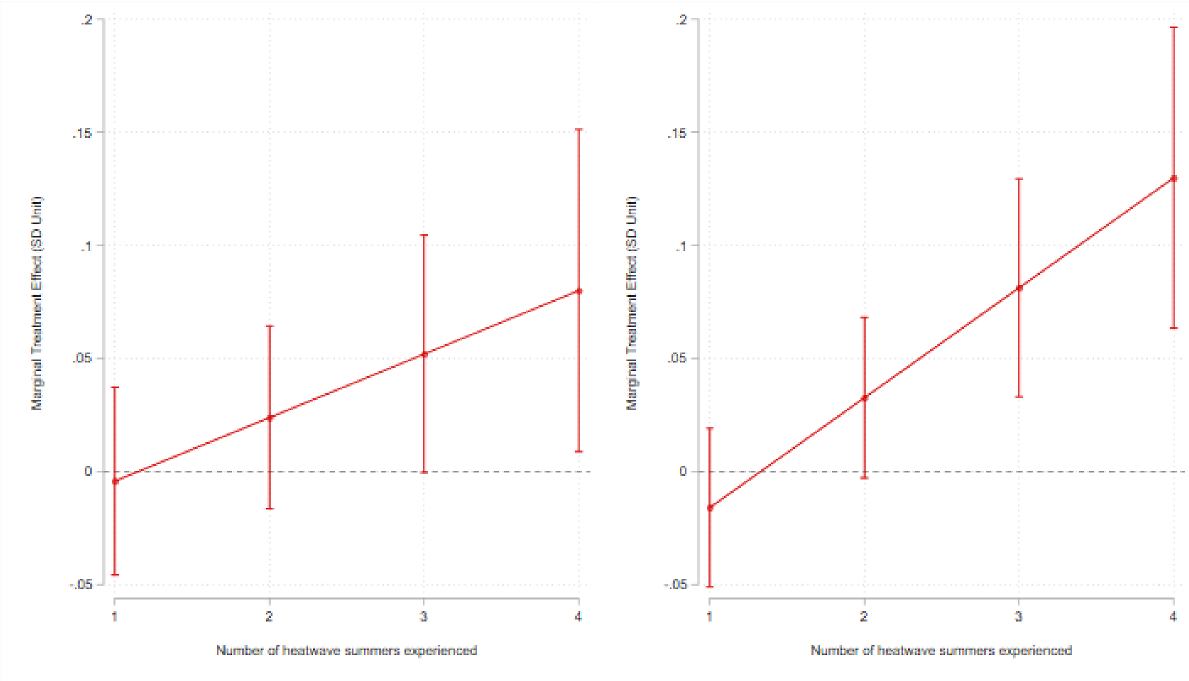

Figure 2.4.7: Marginal treatment effect (SD change) of heatwave exposure with increasing number of heatwave summers experienced

Note: Outcome: Standardised Concern Index (left), $N = 15,000$; Standardised Behaviour Index (right), $N = 26,935$. Error bars indicate 95% confidence intervals.

Taken together, the findings suggest that increases in heatwave frequency (but not flooding) are clearly associated with climate change belief updating. Individuals who experienced multiple heatwave events increased both their climate change concern and pro-environmental behaviour.

2.5 Discussion and conclusion

It has previously been argued that bringing climate change impacts closer to home (i.e. reducing psychological distance to the issue of climate change) may be a promising strategy to increase engagement with the issue and motivation to act (Demski et al., 2017; Reser et al., 2014; Spence et al., 2011; van der Linden et al., 2015). This compelling argument, founded in psychological and economic theories, has inspired both empirical and experimental research into the proposed relationship between 'climate change proximity' and beliefs and engagement. However, the existing evidence is mixed and has frequently suffered from methodological drawbacks (Howe, 2021; Howe et al., 2019). In this chapter, we link geo-referenced climate change opinion data with records of extreme weather events in England and Wales to explore this premise as a form of natural experiment. We find that personal experience with both flooding and heatwaves increases climate change risk perception (belief that climate change will affect people in the UK within the next 30 years) but has no effect on climate change concern and pro-environmental behaviour, on average. However, we also show that average treatment effects mask some important nuanced effects of extreme event exposure, which provide interesting insights into when and how personal experience changes climate change attitudes.

For flooding, our main results are in line with the most recent literature (Albright & Crow, 2019; Demski et al., 2017; Frondel et al., 2017; Osberghaus & Fugger, 2018). We find that experiencing a flood within a 1000-metre radius from the household increases risk perception. However, we find no evidence that flood experience leads to changes in climate change concern or stated pro-environmental behaviour, on average. We further explore the existence of a distance decay gradient between flood experience and risk perception. Our results show that the treatment effect largest for individuals living closest to the flood extent and subsequently decreases as distance increases. We find that this relationship is best explained by a linear distance decay function and that the effect is highly localised: At distances greater than 1000 meters the change in risk perception is no longer statistically distinguishable from zero, which points to the importance of employing geo-referenced data when exploring distance effects. Moreover, this finding suggests that proximity to flooding is directly associated with psychological distance with respect to risk perception. Although it remains challenging to draw conclusions on the underlying mechanisms, a plausible explanation for our findings is that the closer an event is to home, the more personally relevant and memorable it may be (Brügger et al., 2021). Moreover, flood victims may be more likely to establish a cognitive link between the flooding event and climate change and therefore perceive climate change as more certain, temporally close and personally relevant (Ogunbode et al., 2020).

An alternative explanation, often proposed in the literature, is that the relationship between flood exposure and risk perceptions is mediated by negative emotive responses to the flood, which in turn may negatively impact engagement and pro-environmental behaviour (Brügger et al., 2015). We explore this hypothesis for physical flood proximity using self-reported life satisfaction as a proxy for 'emotive responses'. Our results indicate that there appears to be no relationship between flood proximity and either life satisfaction, or pro-environmental behaviour. Interestingly, our analysis of temporal proximity (months since the flood event) using the same outcome variables finds tentative support for this hypothesis. The results show a statistically significant non-linear (inverse U-shaped) effect of temporal proximity on both pro-environmental behaviour and self-reported life satisfaction. Individuals who were interviewed within six months from the flood event reported lower average life-satisfaction and stated pro-environmental behaviour. The negative effect subsequently diminishes and turns positive the more months have passed since the flood event. This finding suggests that individuals initially suffer utility losses from flood exposure, echoing previous findings (Luechinger & Raschky, 2009), which however attenuate over time. Flood victims appear to be reluctant to adopt effortful pro-environmental behaviours immediately after the flood event, however, this effect appears to reverse once sufficient time has passed to mentally recover from the event.

While temporal proximity to the event appears to have a non-linear effect on pro-environmental behaviour, which may relate to the emotional consequence of flood experience, others have argued that the impact of extreme event exposure on climate change attitudes is generally short-lived. This line of reasoning argues that changes are primarily driven through a salience effect, as people assign greater weight to more recent events. We find partial support for this hypothesis in our data. Temporal proximity of the experienced flood event has a linear effect on risk perceptions: the effect is largest for individuals interviewed closest to the flood event, and subsequently diminishes over time. This finding suggests that increases in risk perceptions are likely driven by a salience effect, which is largest in the months succeeding the flood events. However, changes in risk perception remain positive and statistically significant even for people interviewed up to three years after the event occurred, suggesting that while a temporal decay may exist, the effects are not quite a short-lived as previously thought.

With respect to heatwaves, our results suggest that experiencing a heatwave (at least three consecutive days of temperatures > 29°C) increases climate change risk perceptions but has no effect on climate change concern and pro-environmental behaviour, on average. Our main result is consistent with previous studies which find that heatwave exposure is strongly correlated with subjective risk perceptions (Dai et al., 2015; Frondel et al., 2017) but has no effect on pro-environmental behaviour (Larcom et al., 2019). However, our results also uncover more nuanced effects of heatwave exposure. First, we show that changes in risk perception do not appear to be driven by a salience effect. Our analysis of temporal proximity finds no statistically significant relationship between risk perception and the number of months passed since the event. This finding stands in contrast to the previous literature (Larcom et al., 2019). Second, we find that exposure to multiple heatwaves causes a significant increase in both climate change concern and pro-environmental behaviour. This important finding suggests that increasing frequency of heatwaves may aid autonomous adaptation to climate change in the future if heatwave exposure creates a positive feedback loop with engagement and behaviour. In contrast, we find no such frequency effect for flood exposure.

Notwithstanding the numerous methodological innovations, our analysis is not without limitations. van der Linden (2014) points out the importance of cognitive attribution in order for an affective reaction to occur, which is supported by recent empirical evidence (Ogunbode et al., 2019). While we test several pathways, which may strengthen attribution (e.g. event intensity, temporal proximity), we are unable to certify that respondents did in fact draw a cognitive link between the extreme weather event and climate change. In addition, it is important to point out that the analysis relied entirely on self-reported (stated) climate change attitudes and pro-environmental behaviour. Especially with respect to the latter, stated behaviour does not necessarily reflect real-world behaviour, hence we cannot draw any conclusions about *actual* behaviour change. Future research should aim to explore observed behaviours (such as recently Osberghaus and Demski (2019)) to provide evidence for real-world behaviour change.

The findings discussed above entail a number of important policy implications. Although no single event can be directly attributed to climate change, the incidence of severe flooding and heatwave events could be harnessed to raise awareness towards future climate change risks, increasing not only the geographic relevance, but also the temporal proximity. While flood events do not appear to have a direct impact on climate change concern and pro-environmental behaviour, they may provide favourable conditions for climate change communications ain the months after the event. We recommend that risk communication campaigns in the wake of flood events should focus on the geographic proximity of events and highlight the link between the event and climate change to facilitate attribution.

A second key insight from our analysis is the potential of drawing attention to climate change by highlighting the unusual frequency of heatwave events. With intensity and frequency of heatwaves predicted to increase further with global warming (Christidis et al., 2019), our results show that this may prove a promising strategy to not only raise climate change concern but also encourage sustainable behaviours. However, recent evidence from the US shows a rapid decline in the perceived remarkability of extreme temperatures among the general public (Moore et al., 2019). If heatwave events will soon be considered the "new normal", it may imply a limited window of opportunity to highlight the abnormality of increasingly frequent heatwave events. It remains to be seen whether social normalisation of extreme heat conditions will occur at a similar pace in the UK.

In sum, the findings of this chapter suggest that it is reasonable to assume that experiencing the impacts of climate change will reduce the psychological distance to climate change for people in the UK, by increasing personal relevance and perceived risk. However, on average, extreme events will have little effect on the level of engagement and action for most people. Nonetheless, increasingly frequent heatwaves may have a somewhat "self-correcting" effect on psychological distance to climate change and may even motivate behaviour changes. Highlighting the unusual frequency of heatwaves in climate change communications and drawing attention to their anthropogenic cause appears to be a promising strategy to increase concern around climate change and in turn garner support for mitigation policies.

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Appendix

Appendix 2.A Additional tables and figures

Tables

Table 2.A1: UKHLS variables: Climate change questionnaire

Dimension	UKHLS Code	UKHLS Wave	Description
Risk Perception	scopecl30	1,4,10	Do you believe that people in the UK will be affected by climate change in the next 30 years
Climate Change Concern	scenv_bccc	4,10	The effects of climate change are too far in the future to really worry me
	scenv_pmep	4,10	The so-called 'environmental crisis' facing humanity has been greatly exaggerated
	scenv_meds	4,10	Climate change is beyond control - it's too late to do anything about it
	scenv_crex	4,10	If things continue on their current course, we will soon experience a major environmental disaster
	scenv_tlat	4,10	Any changes I make to help the environment need to fit in with my lifestyle
	scenv_nowo	4,10	I don't believe my behaviour and everyday lifestyle contribute to climate change
	scenv_fitl	4,10	I would be prepared to pay more for environmentally friendly products
	scenv_noot	4,10	It's not worth me doing things to help the environment if others don't do the same
	scenv_canc	4,10	It's not worth Britain trying to combat climate change, because other countries will just cancel out what we do
Environmental Behaviour	envhabit1	1,4,10	environmental habits: tv
	envhabit2	1,4,10	Switch off lights in rooms that aren't being used
	envhabit3	1,4,10	Keep the tap running while you brush your teeth
	envhabit4	1,4,10	environmental habits: heating
	envhabit5	1,4,10	environmental habits: packaging
	envhabit6	1,4,10	environmental habits: recycled paper
	envhabit7	1,4,10	environmental habits: shopping bags
	envhabit8	1,4,10	Use public transport (e.g. bus, train) rather than travel by car
	envhabit9	1,4,10	Walk or cycle for short journeys less than 2 or 3 miles
	envhabit10	1,4,10	Car share with others who need to make a similar journey
	envhabit11	1,4,10	Take fewer flights when possible

Note: This table provides an overview of the UKHLS variables used to construct the main dependent variables: risk perception, climate change concern and pro-environmental behaviour. UKHLS Wave refers to the survey waves in which the variables were measured. Description provides the question text used in the UKHLS self-completion questionnaire.

Table 2.A2: Main results

	Risk Perception		Concern Index		Behaviour Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Flood Event	0.066**		0.025		-0.006	
	(0.031)		(0.030)		(0.025)	
Heatwave Event		0.063***		0.009		0.010
		(0.021)		(0.020)		(0.017)
R^2-Adjusted	0.307	0.317	0.623	0.638	0.534	0.531
R^2-Within	0.001	0.001	0.000	0.000	0.000	0.000
Individuals	4,791	9,305	3,925	7,500	5,281	10,590
Observations	11,990	23,109	7,850	15,000	13,656	26,935

Note: OLS estimates of equation (2.1) with the matched sample. The dependent variable in columns (1) and (2) is a binary variable for climate change risk perception. The dependent variable in columns (3) and (4) is an index of climate change concern. The dependent variable in columns (5) and (6) is an index of pro-environmental behaviour. Flood Event and Heatwave Event are the difference-in-differences estimators capturing the treatment effect of flood or heatwave exposure, respectively. Flood treatment is defined as living within a 1000-meter radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29°C. All models include individual and wave-by-season fixed effects. Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.A3: Main results robustness check

	Risk Perception		Concern Index		Behaviour Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Flood Event	0.059*		0.024		0.005	
	(0.032)		(0.030)		(0.026)	
Heatwave Event		0.068***		0.020		0.016
		(0.023)		(0.020)		(0.018)
Individuals	4,791	9,305	3,925	7,500	5,281	10,590
Observations	11,990	23,109	7,850	15,000	13,656	26,935

Note: Estimates of equation (2.1) with the matched sample using aggregation procesdure developed by Callaway and Sant'Anna (2021). The dependent variable in columns (1) and (2) is a binary variable for climate change risk perception. The dependent variable in columns (3) and (4) is an index of climate change concern. The dependent variable in columns (5) and (6) is an index of pro-environmental behaviour. Flood Event and Heatwave Event are the difference-in-differences estimators capturing the treatment effect of flood or heatwave exposure, respectively. Flood treatment is defined as living within a 1000-meter radius from a flood extent and heatwave treatment is defined as having experienced at least three consecutive days of daily maximum temperatures greater than 29°C. All models include individual and wave-by-season fixed effects. Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.A4: Flood proximity

	Risk Perception		Concern Index		Behaviour Index		Life Satisfaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event	0.121***	0.044	0.029	0.011	-0.033	-0.017	0.013	0.036
	(0.042)	(0.065)	(0.040)	(0.058)	(0.036)	(0.055)	(0.061)	(0.095)
Event x MinDis	-0.008**	0.013	-0.003	0.002	0.004	-0.000	-0.002	-0.008
	(0.004)	(0.015)	(0.003)	(0.013)	(0.003)	(0.012)	(0.005)	(0.020)
Event x MinDis^2		-0.001		-0.000		0.000		0.000
		(0.001)		(0.001)		(0.001)		(0.001)
R^2-Adjusted	0.324	0.324	0.634	0.634	0.539	0.539	0.355	0.355
R^2-Within	0.001	0.001	0.000	0.000	0.000	0.000	0.000	0.000
Individuals	7,527	7,527	6,039	6,039	8,385	8,385	7,705	7,705
Observations	18,712	18,712	12,078	12,078	21,503	21,503	19,263	19,263

Note: OLS estimates of equation (2.2). The dependent variable in columns (1) and (2) is a binary variable for climate change risk perception. The dependent variable in columns (3) and (4) is an index of climate change concern. The dependent variable in columns (5) and (6) is an index of pro-environmental behaviour. The dependent variable in columns (7) and (8) is a continous variable capturing self-reported satisfaction with life. Event is the difference-in-differences estimator capturing the treatment effect of flood exposure within a radius of 2000 meters. MinDis and MinDis^2 interacted with the DID indicator (Event) capture the linear and quadratic effect of a 100m increase in the minimum recorded distance to the flood event for treated individuals, respectively. All models include individual and wave-by-season fixed effects. Standard errors clustered at the individual level in parentheses.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.A5: Heatwave duration

	Risk Perception		Concern Index		Behaviour Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Event	0.118*	0.326	0.095	-0.257	-0.053	-0.403**
	(0.066)	(0.222)	(0.060)	(0.205)	(0.056)	(0.185)
Event x MaxDur	-0.012	-0.110	-0.020	0.150	0.014	0.179**
	(0.014)	(0.101)	(0.013)	(0.095)	(0.012)	(0.084)
Event x MaxDur^2		0.011		-0.019*		-0.018**
		(0.011)		(0.011)		(0.009)
R^2-Adjusted	0.317	0.317	0.638	0.638	0.531	0.531
R^2-Within	0.001	0.001	0.000	0.001	0.000	0.000
Individuals	9,305	9,305	7,500	7,500	10,590	10,590
Observations	23,109	23,109	15,000	15,000	26,935	26,935

Note: OLS estimates of equation (2.2). The dependent variable in columns (1) and (2) is a binary variable for climate change risk perception. The dependent variable in columns (3) and (4) is an index of climate change concern. The dependent variable in columns (5) and (6) is an index of pro-environmental behaviour. Event is the difference-in-differences estimator capturing the treatment effect of heatwave exposure. MaxDur and MaxDur^2 interacted with the DID indicator (Event) capture the linear and quadratic effect of a one day increase in the maximum number of heatwave days experienced by treated individuals, respectively. All models include individual and wave-by-season fixed effects. Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.A6: Temporal proximity: Flooding

	Risk Perception		Concern Index		Behaviour Index		Life Satisfaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event	0.142***	0.105	0.039	0.085	0.035	-0.082	-0.088	-0.189*
	(0.050)	(0.074)	(0.044)	(0.073)	(0.040)	(0.055)	(0.070)	(0.101)
Event x Months	-0.002*	0.000	-0.000	-0.003	-0.001	0.006**	0.002	0.008*
	(0.001)	(0.004)	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.005)
Event x Months^2		-0.000		0.000		-0.000***		-0.000
		(0.000)		(0.000)		(0.000)		(0.000)
R^2-Adjusted	0.307	0.307	0.623	0.623	0.534	0.534	0.357	0.358
R^2-Within	0.001	0.001	0.000	0.000	0.000	0.001	0.000	0.000
Individuals	4,791	4,791	3,925	3,925	5,281	5,281	4,893	4,893
Observations	11,990	11,990	7,850	7,850	13,656	13,656	12,325	12,325

Note: OLS estimates of equation (2.3). The dependent variable in columns (1) and (2) is a binary variable for climate change risk perception. The dependent variable in columns (3) and (4) is an index of climate change concern. The dependent variable in columns (5) and (6) is an index of pro-environmental behaviour. The dependent variable in columns (7) and (8) is a continous variable capturing self-reported satisfaction with life. Event is the difference-in-differences estimator capturing the treatment effect of flood exposure. Months and Months^2 interacted with the DID indicator (Event) capture the linear and quadratic effect of a one month increase in the number of months between the flood event and the interview date, respectively. All models include individual and wave-by-season fixed effects. Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.A7: Temporal proximity: Heatwaves

	Risk Perception		Concern Index		Behaviour Index		Life Satisfaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event	0.070***	0.077***	0.005	0.009	0.019	0.011	0.099***	0.076*
	(0.024)	(0.029)	(0.023)	(0.028)	(0.020)	(0.024)	(0.037)	(0.043)
Event x Months	-0.001	-0.002	0.000	-0.000	-0.001	0.000	-0.007***	-0.003
	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)	(0.003)	(0.005)
Event x Months^2		0.000		0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)		(0.000)
R^2-Adjusted	0.317	0.317	0.638	0.638	0.531	0.531	0.340	0.340
R^2-Within	0.001	0.001	0.000	0.000	0.000	0.000	0.001	0.001
Individuals	9,305	9,305	7,500	7,500	10,590	10,590	9,514	9,514
Observations	23,109	23,109	15,000	15,000	26,935	26,935	23,784	23,784

Note: OLS estimates of equation (2.3). The dependent variable in columns (1) and (2) is a binary variable for climate change risk perception. The dependent variable in columns (3) and (4) is an index of climate change concern. The dependent variable in columns (5) and (6) is an index of pro-environmental behaviour. The dependent variable in columns (7) and (8) is a continous variable capturing self-reported satisfaction with life. Event is the difference-in-differences estimator capturing the treatment effect of heatwave exposure. Months and Months^2 interacted with the DID indicator (Event) capture the linear and quadratic effect of a one month increase in the number of months between the heatwave event and the interview date, respectively. All models include individual and wave-by-season fixed effects. Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.A8: Event frequency: Flooding

	Risk Perception		Concern Index		Behaviour Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Event	0.148***	0.265*	0.023	0.104	0.001	0.039
	(0.054)	(0.143)	(0.048)	(0.103)	(0.048)	(0.112)
Event x Count	-0.068*	-0.214	0.002	-0.100	-0.006	-0.054
	(0.037)	(0.170)	(0.033)	(0.118)	(0.033)	(0.136)
Event x Count		0.035		0.024		0.012
		(0.041)		(0.027)		(0.033)
R^2-Adjusted	0.307	0.307	0.623	0.623	0.534	0.534
R^2-Within	0.001	0.001	0.000	0.000	0.000	0.000
Individuals	4,791	4,791	3,925	3,925	5,281	5,281
Observations	11,990	11,990	7,850	7,850	13,656	13,656

Note: OLS estimates of equation (2.4). The dependent variable in columns (1) and (2) is a binary variable for climate change risk perception. The dependent variable in columns (3) and (4) is an index of climate change concern. The dependent variable in columns (5) and (6) is an index of pro-environmental behaviour. Event is the difference-in-differences estimator capturing the treatment effect of flood exposure. Months and Months^2 interacted with the DID indicator (Event) capture the linear and quadratic effect of a one unit increase in the number of floods experienced by treated individuals, respectively. All models include individual and wave-by-season fixed effects. Standard errors clustered at the individual level in parentheses.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Table 2.A9: Event frequency: Heatwaves

	Risk Perception		Concern Index		Behaviour Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Event	0.045	0.076	-0.032	-0.000	-0.065***	-0.059
	(0.030)	(0.075)	(0.028)	(0.075)	(0.024)	(0.061)
Event x Count	0.012	-0.023	0.028**	-0.007	0.049***	0.042
	(0.013)	(0.078)	(0.013)	(0.078)	(0.012)	(0.065)
Event x Count		0.008		0.008		0.001
		(0.018)		(0.018)		(0.015)
R^2-Adjusted	0.317	0.317	0.638	0.638	0.532	0.532
R^2-Within	0.001	0.001	0.001	0.001	0.001	0.001
Individuals	9,305	9,305	7,500	7,500	10,590	10,590
Observations	23,109	23,109	15,000	15,000	26,935	26,935

Note: OLS estimates of equation (2.4). The dependent variable in columns (1) and (2) is a binary variable for climate change risk perception. The dependent variable in columns (3) and (4) is an index of climate change concern. The dependent variable in columns (5) and (6) is an index of pro-environmental behaviour. Event is the difference-in-differences estimator capturing the treatment effect of heatwave exposure. Months and Months^2 interacted with the DID indicator (Event) capture the linear and quadratic effect of a one unit increase in the number of heatwave summers experienced by treated individuals, respectively. All models include individual and wave-by-season fixed effects. Standard errors clustered at the individual level in parentheses.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Figures

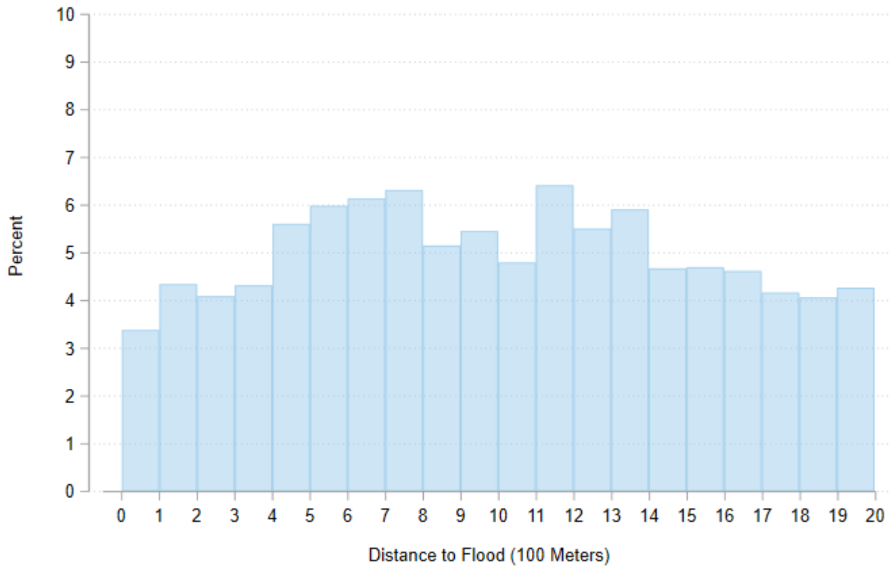

Figure 2.A1: Sample distribution: Distance to flood; *Note*: Distance = 2,000 m, $N = 3,460$

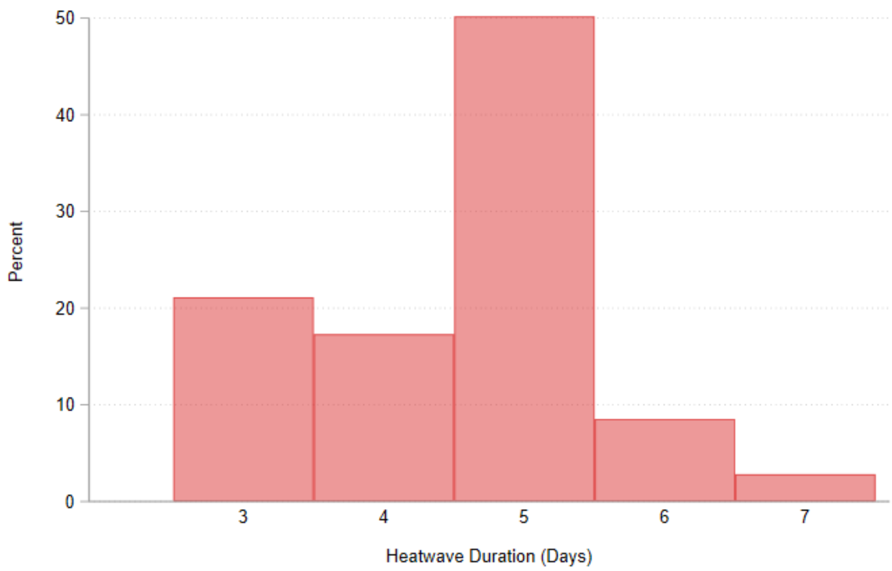

Figure 2.A2: Sample distribution: Heatwave duration; *Note*: $N = 6,660$

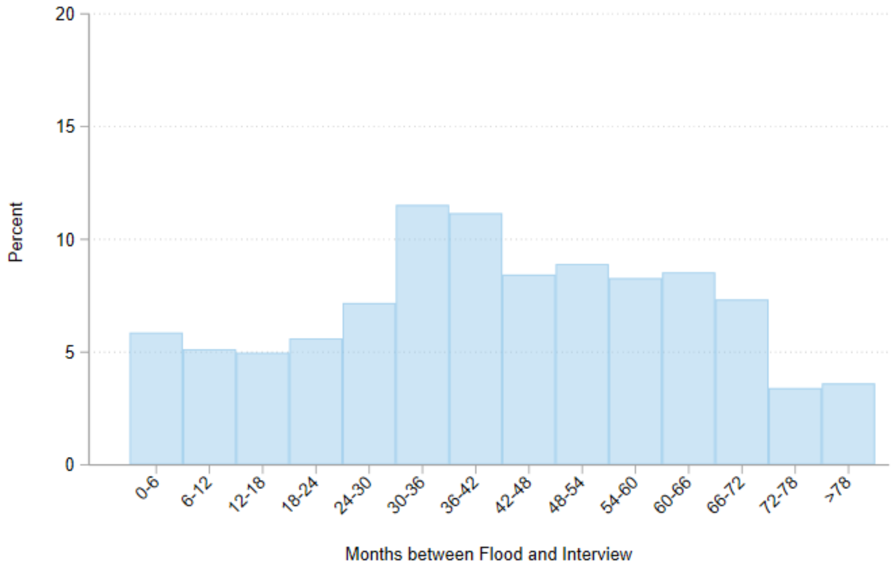

Figure 2.A3: Sample distribution: Temporal proximity (flooding); *Note*: $N = 1,908$

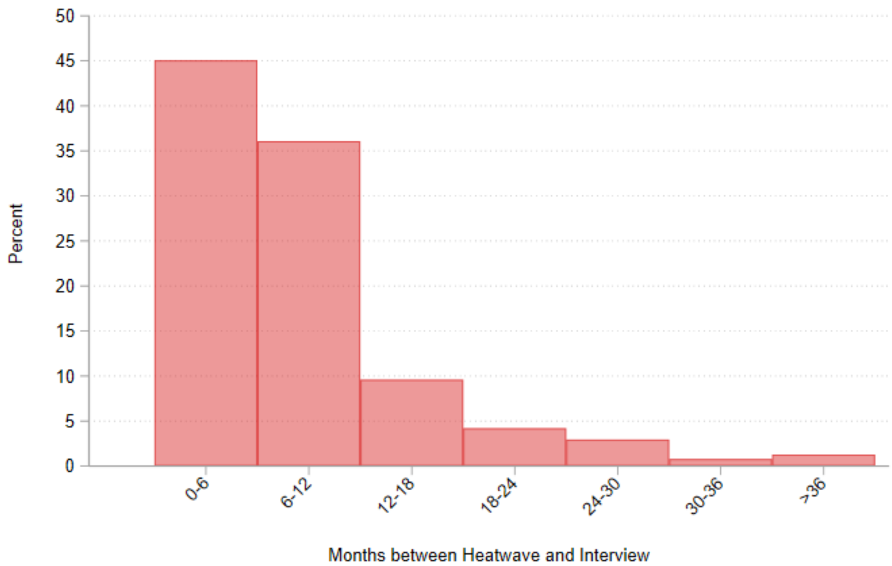

Figure 2.A4: Sample distribution: Temporal proximity (heatwaves); *Note*: $N = 6,660$

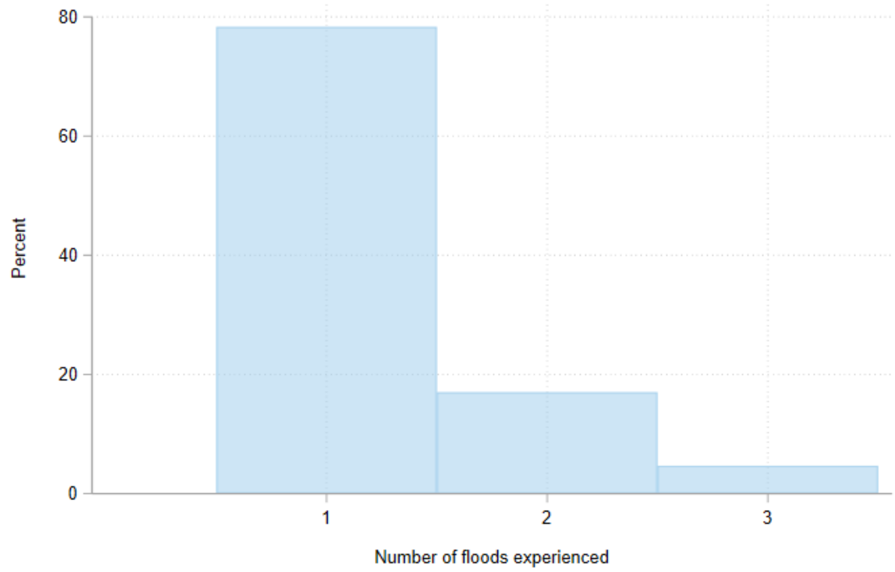

Figure 2.A5: Number of floods; *Note*: $N = 1,908$

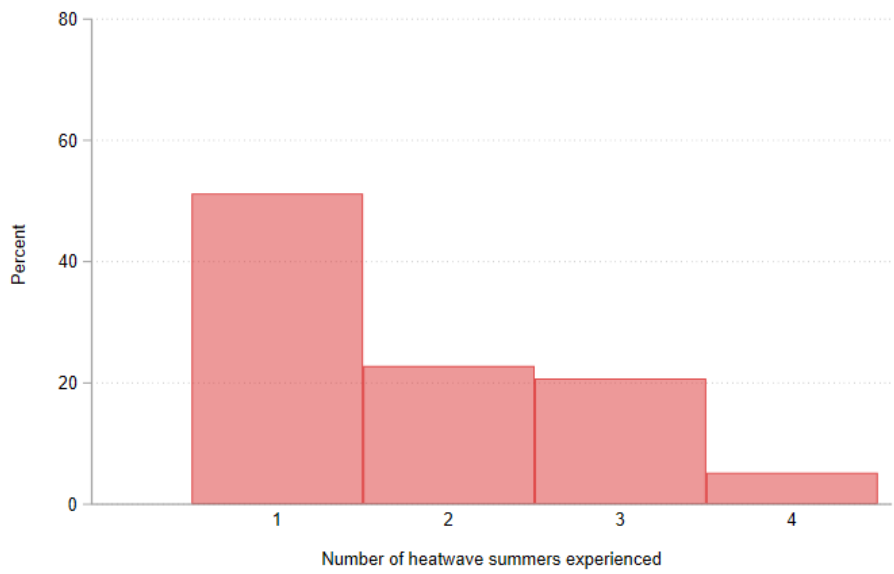

Figure 2.A6: Number of heatwaves; *Note*: $N = 6,660$

Chapter 3

Turning up the heat: Encouraging pro-environmental behaviour through warm glow

3.1 Introduction

Encouraging pervasive sustainable behaviour change, beyond mere intentions, remains one of the most pressing challenges for public policy. Previous approaches have heavily relied on incentives and appeals targeting people's extrinsic motivation, including economic incentives directly rewarding sustainable behaviour or more abstract rewards such as social recognition. However, extrinsically motivated interventions have often failed to achieve long-lasting behaviour change (Frey & Rogers, 2014; Gravert & Olsson, 2021; Kaiser et al., 2020). It has been argued that for pro-environmental behaviour to be sustained in the long term, it needs to be internalised and thus motivated by intrinsic factors (Steinhorst & Klöckner, 2017). Therefore, appealing to people's intrinsic motivation may be a more promising strategy to promote long-run sustainable actions (Steg, 2016; Taufik et al., 2015; van der Linden, 2015, 2018). In this regard, increasing evidence confirms that highlighting the intrinsic motivational basis of pro-environmental behaviour is more effective than emphasising monetary gains (Asensio & Delmas, 2015; Bolderdijk et al., 2013; D. Schwartz et al., 2015; Steinhorst & Klöckner, 2017).

Theoretically founded in Schwartz's (1977) norm-activation theory, intrinsic motivation stems from the personal desire to act morally and in turn avoid negative moral emotions such as guilt and shame (Onwezen et al., 2013). In line with this reasoning, normative appeals to encourage sustainable behaviour have typically focused on arousing negative self-focused moral emotions or a guilty conscience (Rees et al., 2015). Contrarily, another important source of intrinsic motivation towards sustainable behaviour is the positive emotional reward (or 'warm glow') from acting in-line with one's moral values (van der Linden, 2015). The theory of warm glow and impure altruism, first formalised by James Andreoni (1989, 1990), suggests that people obtain positive utility from the act of helping others, which serves as a key motivator of pro-social behaviour (Andreoni, 1995; Crumpler & Grossman, 2008; Ferguson & Flynn, 2016; Konow, 2010; Ottoni-wilhelm et al., 2017; Tonin & Vlassopoulos, 2014, 2010). In the environmental domain, increasing evidence testifies that warm glow is an important predictor of sustainable behaviour, however, it remains underexplored whether warm glow experiences can be exogenously manipulated and leveraged to motivate sustainable behaviour (Hartmann et al., 2017; Kácha & Ruggeri, 2018; Taufik et al., 2015; van der Linden, 2018). One approach may be to directly appeal to people's warm glow motives. In non-environmental contexts, recent field evidence suggests that informational cues about warm glow experiences have the potential to foster cooperation and increase pro-social behaviour (Bergquist et al., 2020; Ferguson et al., 2020; List et al., 2021; Neumann, 2019). However, it is not yet known whether these findings are transferable to sustainable behaviours. Moreover, it has often been argued that creating opportunities to experience warm glow from sustainable behaviour could initiate a positive feedback loop in which previously experienced warm glow gives rise to anticipated warm glow, thus motivating future pro-environmental behaviour (Brosch, 2021; Schneider et al., 2021; van der Linden, 2018). For instance, Hartmann et al. (2017) show that warm glow can both drive pro-environmental behaviour and reinforce intentions to engage in future pro-environmental behaviour, however, robust evidence that positive reinforcement occurs with actual behaviour is lacking.

In this study, we utilise a large-scale pre-registered online experiment to evaluate the effectiveness of four different messaging interventions, targeting both intrinsic motivation (via emotional reward) and extrinsic motivation (via social reward), in shifting people's willingness to act on climate change.[1] We randomly assign participants to one of four message interventions: (1) a warm glow appeal, which highlights the positive emotional reward from helping the environment, (2) a cold prickle appeal, which highlights the negative moral emotions of not helping the environment, (3) a social norm appeal which communicates a prescriptive (injunctive) norm and (4) a baseline condition in which only basic information

[1]Ethical approval for the experiment was granted by the Department of Land Economy Ethical Research Committee. The study was pre-registered on the Open Science Framework (osf.io) prior to data collection, June, 2021. https://osf.io/gbmv7.

on climate change is presented. Notably, all four messages hold constant the call to action on climate change, and vary only the salience of emotions and the social norm around PEB. To increase (emotional) engagement, treatment messages were administered in the form of short explainer-style animated videos. We then quantify anticipated and experienced emotions derived from the cognitive appraisal of contributing towards environmental protection, which serve as measures of "warm glow" and "cold prickle" emotions. To elicit willingness to act on climate change, we develop a novel incentivized paradigm on pro-environmental effort. Moreover, we explore the persistency of our messaging intervention by measuring pro-environmental effort two days after the main experimental survey.

Our study design introduces several key innovations: First, we introduce a novel incentive-compatible experimental measure of people's willingness to act pro-environmentally. Participants conduct a real-effort task that is tedious and thus resembles pro-environmental effort, which often implies personal costs and/or demands extra effort. The greater a participant's effort in the task, the larger the donation amount generated for Friends of the Earth, an environmental charitable organization that implements various projects on climate change mitigation and environmental protection. The measure is quantitative and allows us to observe time invested ('quantity') and actual performance on the task ('quality'). Our novel measure of PEB thus improves on previous research in lab settings, which has relied primarily on measures of behavioural intentions (i.e., self-reports of PEB) or simple donations derived from windfall gains to measure pro-environmental behaviour (Schneider et al., 2021). Second, we utilise a longitudinal experimental design to explore the persistence of behavioural change in response to message interventions, thus filling an important gap in the extant literature. Third, our controlled experimental setting allows us to measure self-reported emotions and explicitly test whether affective responses mediate the relationship between our message interventions and PEB. Our findings thus provide an important complement to research on warm glow appeals where emotions cannot be explicitly observed.

Our results indicate that appealing to warm glow motives was unsuccessful in increasing pro-environmental effort, relative to a control group which received only basic information on climate change. Both message frames directly appealing to the warm glow and cold-prickle emotions were only partially successful in manipulating anticipated affect, if compared to the baseline condition. Contrary to our expectations, warm glow framing did not raise anticipated positive affect and cold-prickle framing did not increase anticipated negative affect. In line with our expectations, we find that emotions were largely unchanged by social norm framing. Subsample analysis distinguishing between people with higher and lower levels of biospheric values suggests that cold-prickle framing reduced donations amongst individuals with lower biospheric values and warm-glow messaging had a negative effect on individuals with strong biospheric values. This important finding suggests that climate change communications appealing to both negative and positive emotions may "backfire" for certain people.

To provide insights into the persistency of emotive and social norm appeals, we examined whether any of the treatment messages influenced donation behaviour two days after the first experimental survey wave utilising the identical experimental design but removing the treatment stimulus. The results from this analysis suggest that donation behaviour is largely unchanged over time and find no statistically significant differences across waves and treatment conditions.

3.1.1 Research questions and hypotheses

As pre-specified (see https://osf.io/gbmv7), we address the following four research questions: (1) Do intrinsically or extrinsically motivated message frames influence the anticipated and experienced emotions of engaging in PEB, relative to a baseline condition that receives only basic information on climate change? (2) Do warm glow, cold-prickle and social norm messages increase PEB relative to the baseline condition? (3) Do emotions mediate the relationship between treatment messages and PEB? (4) Does the effect of treatment messages on PEB persist over time?

Our first research question explores whether there is a main effect of the treatment interventions on pro-environmental behaviour relative to the baseline condition. To that end, we examine the relative effectiveness of different message frames in encouraging PEB. We hypothesised that all treatment messages would increase PEB relative to the baseline condition and that the warm glow message, increasing the salience of benefits-to-self, would perform best.

Our second research question asks whether intrinsically motivated messages increasing the salience of warm glow and cold-prickle emotions, or a descriptive social norm message influence anticipated and experienced emotions, relative to a baseline condition which received only basic information on climate change. We hypothesised that warm glow messaging, which highlights the positive emotional reward of PEB, would be positively associated with positive affect and negatively with negative affect. We assumed the opposite to be the case for cold-prickle messaging, which increased the salience of negative emotions (e.g., guilt and shame) from failing to act pro-environmentally. We did not expect to find a relationship between the social norm message and emotions.

The third research question examines whether anticipated and experienced emotions mediate the relationship between the treatment messages and pro-environmental behaviour. We hypothesised that anticipated positive emotions would partially mediate the relationship between the warm glow message and PEB. Similarly, we expected the cold prickle message to impact PEB via negative emotions.

Our fourth and final research question explores the persistency of treatment effects and thus contributes to an emerging literature which has largely highlighted the impermanence of behavioural interventions (Allcott & Rogers, 2014; Brandon et al., 2017; Gravert & Olsson, 2021). Specifically, we hypothesised that PEB would decrease in the second experimental wave (T2), however, to a lesser extent in the warm glow group. Moreover, we expected positive emotions to be higher in the warm glow group at T2.

In addition to our pre-registered hypotheses, we conduct additional exploratory analysis. First, we explore the relationship between value orientation (biospheric and altruistic) and PEB, and test to what extent this relationship is mediated by anticipated warm glow. Second, we investigate whether warm glow experienced after engaging in pro-environmental behaviour mediates future pro-environmental behaviour (Brosch, 2021). The exploratory analysis allows us to examine whether pro-environmental behaviour and experienced emotions can form a positive self-reinforcing feedback loop with each other over time.

3.1.2 Contributions

This chapter contributes to the aforementioned literature and extends previous work along multiple dimensions. First and foremost, our study contributes to the emerging literature on warm glow as an important motivator of pro-environmental behaviour (Gråd et al., 2021; Hartmann et al., 2017; Taufik, 2018; Taufik et al., 2015; van der Linden, 2018). More generally, this research falls within an emerging literature on (positive) emotions and climate change engagement (Brosch, 2021; Schneider et al., 2021). Schneider and colleagues (2021) review the recent literature around a framework of positive emotions as antecedents and consequences of climate-change related engagement and conclude that more research is required to explore actual behaviour (rather than intentions), using large-scale longitudinal studies (rather than cross-sectional designs looking at short-term individual pro-environmental actions). Our study addresses these major gaps, by implementing a longitudinal design using a large online RCT. Moreover, recent research has stressed the potential for positive emotions (specifically warm glow) to form a positive feedback-loop with climate change engagement (Brosch, 2021; Schneider et al., 2021; van der Linden, 2018). Our longitudinal experimental design allows us to explore whether appealing to intrinsic motives can kick-start such a self-reinforcing 'virtuous cycle'. Moreover, our study attempts to address the long-standing challenge of whether warm glow experiences can be exogenously manipulated in a controlled experimental setting (Hartmann et al., 2017).

Zooming out further, this chapter speaks to the literature exploring the intrinsic motivational basis for PEB (Gråd et al., 2021; Kácha & Ruggeri, 2018; Steg, 2016; van der Linden, 2015; Venhoeven et al., 2020). Our results shed light on whether appealing to intrinsic motives via moral emotions such as warm glow and cold prickle performs differently from social norm messaging, which may be considered an extrinsic motivator. We extend this literature by explicitly measuring anticipated and experienced emotions, both before and after engaging in PEB. Our findings thus contribute to the broader research agenda in psychology and economics aimed at understanding the drivers of individual pro-environmental behaviour and voluntary climate action (Diederich & Goeschl, 2014; Steg, 2018; Steg & Vlek, 2009).

By viewing social norms through the lens of anticipated and experienced emotions, we provide additional insights on the emotional consequences of social messaging in the context of pro-environmental behaviour. While it is well understood that social comparisons are a type of moral nudge which influence behaviour by directly influencing people's (dis)utility from engaging or failing to engage in an action (Carlsson et al., 2021), little consideration has been given to the fact that increasing the salience of social norms may entail substantial negative welfare effects (such as hedonic costs), thus undermining the viability of such policy approaches (Allcott & Kessler, 2019; Carlsson et al., 2021; Trujillo et al., 2021). A full social welfare analysis would require a holistic assessment of the nudge's benefit and costs, considering both behavioural and hedonic outcomes (Allcott & Kessler, 2019). Understanding the emotional response to social norm messaging presents a first step in this direction.

From a methodological perspective, this chapter contributes to the literature utilising lab and online experiments to study PEB. While most experimental studies on PEB conducted in the (online) laboratory have been limited to measuring pro-environmental intentions and one-off donation decisions, our experiment introduces an innovative incentive-compatible real effort task to measure pro-environmental behaviour. Our PEB-paradigm could be readily applied to study other research questions in a controlled experimental setting.

Moreover, the present study contributes to the literature on the optimal design of emotive climate change communications. A current debate in the climate change communication literature is concerned with the relative efficacy of positively (e.g., hope) and negatively (fear, guilt) framed communications (Adams et al., 2020; Bissing-Olson et al., 2016; Charness &

Dufwenberg, 2016; Rees et al., 2015; Schneider et al., 2017). The empirical findings from this literature are inconclusive and suggest that both positive and negative emotions have been successfully leveraged in climate change communications and interventions to increase intentions and actual climate action (Brosch, 2021). We extend this literature by testing the relative efficacy of positive and negative framing, as well as social norm framing, within the same controlled experimental setting using an incentive-compatible measure of PEB. Our findings thus have immediate practical implications for public policy, in particular with respect to the design of online communication campaigns (e.g. via social media channels) aimed at encouraging sustainable behaviour.

Finally, our study contributes to the literature exploring the persistence of behavioural interventions (Allcott & Rogers, 2014; Bernedo et al., 2014; Brandon et al., 2017; Gravert & Olsson, 2021; Hume et al., 2020). Our longitudinal design allows us to study whether treatment effects persist when participants face the same pro-environmental behaviour task, but the stimulus has been removed (i.e., the treatments are not shown again).

3.2 Study design

Data were collected via a pre-registered online experiment, and recruitment of participants took place via the online crowdsourcing platform Prolific Academic (Palan & Schitter, 2018). The study was programmed with the survey software Qualtrics and hosted at the servers of the University of Cambridge. The study consisted of a 3-wave design, including a baseline survey (baseline wave) used for stratified randomisation and assignment to treatment conditions and two experimental surveys (main experiment and a follow-up). Data for all three survey waves were collected on three days during the week of 19th July 2021.[2] To incentivise participation in all parts, participants were informed that upon completion of all parts, they would be sent an additional bonus payment of £1. The following sections will outline the study design in more detail.

3.2.1 Baseline survey and randomisation

The baseline survey served as an introduction to the study and was used to recruit an initial target population of 3,000 individuals which were currently resident in the UK. Participants were informed that they would be participating in a multipart study and would receive an

[2]Note that the study was originally designed and pre-registered to consist of four waves: a baseline survey and three follow-up surveys. Due to unexpected financial constraints, the research team decided to end data collection after the third wave. Foregoing the fourth wave had no impact on the study design or analysis.

invitation to complete surveys on different days in the week of 19th July 2021. After providing informed consent to participate in all parts, participants completed the baseline survey, for which they received a flat participation payment (£1).

The primary objective of the baseline survey was to obtain baseline information on the participants to implement a stratified randomisation procedure for treatment assignment in the main experiment. The survey included standard socio-demographic questions, two questions on subjective well-being and a values questionnaire designed to identify potential subgroups for which heterogeneous treatment effects were expected. The values questionnaire contained egoistic, altruistic and biospheric value orientation measures (de Groot & Steg, 2008).

Moreover, we used the baseline survey as an opportunity to introduce participants to a neutral version of the real-effort task (which will be described in detail below), from which we obtain the experimental outcomes in the main experiment and follow-up surveys. After reading the instructions for the task and being guided through several practice rounds, participants completed the task for 60 seconds with the objective to score as many points as possible. From this task, we obtained the number of correct completions during the allocated timeframe, which serves as a measure of baseline ability for our stratified randomisation procedure. Participants were also asked to report perceived difficulty and enjoyability of the task.[3] At the end of the baseline survey, participants received a reminder of the remaining schedule for the study, indicating the three remaining survey dates.

In total, we recruited 3,001 participants who fully completed the baseline survey. Of these, 21 respondents (0.7%) were excluded as they failed a basic attention check, asking participants to place the slider on a specified number incorporated as an additional item in the values questionnaire. The remaining 2,980 students were then randomly assigned to one of four treatment groups following a stratified sample and re-randomisation procedure implemented using Stata17. Participants were first stratified by gender, baseline ability, past donation

[3]Using a baseline survey to collect baseline information prior to the main experiment has several advantages. First, it allows us to obtain baseline characteristics unconfounded by our treatment intervention. For a discussion of potential posttreatment bias see Montgomery et al. (2018). The following example illustrates where potential bias may arise. If biospheric values were measured after the treatment, these may be biased by the message framing. Moreover, if values were measured immediately prior to the treatment, they may have confounded our treatment messages, as recalling one's own environmental values or past behaviour has itself been used to manipulate pro-environmental behaviour (Panzone et al., 2018). A second advantage of utilising a baseline survey is that unconfounded baseline information can be used to implement a stratified randomisation procedure for the main experiment, which improves balance and statistical power.

behaviour, and general satisfaction with life.[4] Within each stratum, every fourth participant was assigned to a given treatment condition. Following the initial assignment to treatment, balance checks were run using additional sociodemographic variables obtained from the baseline survey. These included basic demographic characteristics (age, education, income, and annual amount (£) donated), baseline altruistic and biospheric values, self-reported eudaemonic well-being (worthwhileness of life) and self-reported difficulty and enjoyability of the Stroop Task. A detailed overview of all variables used for balance checks can be found in Appendix Table 3.A1.

3.2.2 Main experiment

The experimental surveys contain several modules. In the following, we explain our approach to measuring pro-environmental effort, as well as anticipated and experienced emotions. Subsequently, we present the video information treatments embedded in the main experimental survey wave and describe the set-up of the main experiment and follow-up surveys, the latter designed to explore whether the treatments had a persistent effect.

Pro-environmental effort

A long-standing challenge for experimental research on pro-environmental behaviour has been its measurement. The multidisciplinary interest in pro-environmental behaviour has generated a large variety of measurement tools, however, the majority of studies have relied on self-report measures (Lange & Dewitte, 2019). In addition to self-reports, another common approach is to allow participants to donate part of their payoff to an environmental charity at the end of the survey (see e.g. Schneider et al., 2017). While this increases the degree of incentive-compatibility and reflects the trade-off between personal gain and pro-social gain, it is based on a single decision which may not accurately represent real-world pro-environmental behaviour, which is often effortful in addition to costly.

To measure pro-environmental action, we used an incentivized effort-donation paradigm. Participants were asked to complete a real-effort task based on a standard Stroop Task (Stroop, 1935) adapted from McClanahan (2020). Here, participants are shown one of four words (red, green, blue, yellow) randomly printed in one of the four colours and need to use their keyboard to enter the ink-colour of the words independently of the written word. They have a maximum of three seconds per word trial. The task is both cognitively demanding

[4] To decrease the number of strata, we constructed a categorical variable with four categories for both baseline ability and life satisfaction corresponding to equally sized quartiles of the distribution for each of the respective continuous variables assessed in the baseline survey.

and relatively tedious, thus providing an ideal framework to measure pro-environmental effort. Each successfully completed trial generates a donation of 2.5p for Friends of the Earth[5] on behalf of the participants. The more correct trials participants submit, the higher their donations to Friends of the Earth. In total, participants can exert pro-environmental effort for up to 10 minutes, which translates into a maximum of 200 trials of 3 seconds, while they were allowed to quit and thus stop the task at anytime during that 10-minute interval. To incentivise effort, payment to the charity was conditional on correct completions meaning that participants could earn a total of £5 for charity, if they completed all 200 trials correctly.[6] We use the pro-environmental effort task to construct the following four outcome measures for the main analysis:

(1) total donation generated in GBP, (2) share of participants who participated in the voluntary part of the survey and completed at least one trial (3) time spent (quantity dimension) and (4) share of correct trials (quality dimension). Note that the total donation (£) generated serves as our primary outcome variable, as it is directly determined by the time spent on the task and the number of correct completions (i.e., it combines both the time invested and participants' performance).

Treatment messages

Participants were randomly assigned to receive one of four treatment messages. To present the messages in a simple yet engaging manner, the messages were developed into 2D-anmitaed explainer-style videos in partnership with an animation studio (Spark Animation). The videos were between 25 and 56 seconds long and featured animated characters to increase emotional engagement and narrative transportation. The messages were narrated by a professional voice-over artist with a British accent, and subtitles were displayed at the bottom of the video in case participants did not have audio. All videos can be viewed on our designated YouTube channel (https://www.youtube.com/channel/UCm6fSbB-QiYiMx4siHkQ1iQ).

The baseline message (video basic information) was 25 seconds long and presented basic information on the issue of climate change. The script reads as follows:

[1] Basic Information [https://youtu.be/9A5XpweL4Gk]
"*Emissions of carbon dioxide are a primary driver of climate change and present one of the world's*

[5] Friends of the Earth is one of the largest environmental charities in the UK. In a pilot study, we had presented participants with a choice of four UK-registered environmental charities, of which Friends of the Earth was selected as the most popular. It was thus chosen as the default option for the main experiment.

[6] As the task is cognitively challenging, we expected the share of correct completions to be approximately 50%.

most pressing challenges. Did you know? Cutting carbon emissions by half can limit global warming to 1.5°C and reduce the harmful impacts of climate change. Act now by contributing today."

All other treatment videos were up to 56 seconds long, and the introductory paragraph was identical to the above. In addition, the treatment messages included sentences highlighting positive emotions (warm glow video) of helping the environment, negative emotions (cold prickle video) of not helping the environment and the pro-environmental beliefs and behaviours of others (social norm video). The scripts of the warm glow, cold prickle and social norm video included the following additional information:

[2] Warm Glow [https://youtu.be/9A5XpweL4Gk]
"Have you ever experienced that warm, fuzzy feeling when helping others? You may get the same feeling when you make climate friendly choices. People who help the environment often feel uplifted, positive, and experience deep feelings of joy and happiness. When you help the environment, it creates a pleasant feeling known as "warm glow", a rewarding emotion that makes you feel good about your contribution. Helping the environment reduces stress and will boost your well-being. Warm your heart and experience these positive emotions by contributing today."

[3] Cold Prickle [https://youtu.be/Pmsjgmo57Fw]
"Have you ever experienced that guilty feeling when you've let someone down? You may get the same feeling if you make climate damaging choices. People who do not act to help the environment often end up feeling guilty, shameful and regretful. When you fail to help the environment, it creates an unpleasant feeling known as "cold prickle", a negative emotion that makes you feel bad about your inaction. You may end up feeling stressed and unhappy about your choices. You will feel bad about not contributing today."

[4] Social Norm [https://youtu.be/80Iih3l84YI]
"Many people choose to contribute to the global effort to tackle climate change. 8 of 10 people in the UK believe we should do everything necessary, urgently, in response to the climate crisis. Many people's decisions to help the environment have been informed by this social norm, which implies a shared expectation that the majority of people now engage in sustainable behaviour. Do your part by contributing today."

Emotions

As pre-specified, the measures of emotions employed in our study were designed to capture the emotions derived from the act of contributing towards environmental protection. In contrast to existing measures of unspecific situational emotion or mood (such as the Positive and Negative Affect Schedule (Watson, 1988), our measurement items required participants to establish a cognitive link between the emotions elicited and the act of "helping the environment" (Hartmann et al., 2017). Moreover, we asked participants to consider two temporal dimensions, anticipated and experienced emotions:

1. Anticipated emotions were elicited by asking participants to report how helping the environment would make them feel. The measurement items were presented prior to completing the pro-environmental action task and just after the treatment videos had been shown.

2. Experienced emotions were elicited by asking participants to report how helping the environment made our participants feel. The measurement items were presented immediately after completing (or exiting) the pro-environmental effort task.

Five positively framed measurement items (happy, proud, hopeful, inspired, warm) were used to construct a measure of positive affect (or 'warm glow'), which incorporates different dimensions of emotional reward derived from the act of helping the environment. Additionally, five negatively framed items (cold, guilty, anxious, angry, sad) were used to construct a measure of negative affect (or 'cold prickle'), which captures potential negative moral emotions. Participants were asked to rate each of these items on an 11-point scale ranging from 0 (not at all) to 10 (very much). Following our pre-analysis plan, respective positive and negative emotions items were averaged to construct scores for anticipated and experienced positive and negative affect. All four scores ranged from 0 to 10 and achieved an overall satisfactory scale validity as measured by the Cronbach's Alpha (0.92 for anticipated positive affect, 0.82 for experienced positive affect; 0.95 for anticipated negative affect and 0.82 for experienced negative affect).

Experimental procedures

Main experimental survey: The experimental design of the main experimental survey is visualised in Figure 3.2.1. Participants were first shown some generic instructions for the 'key-pressing task' and subsequently completed six practice rounds to familiarise themselves with the task. The first three practice rounds had to be completed correctly, before the participant could proceed, while the second three were completed "at speed" with a three-second time

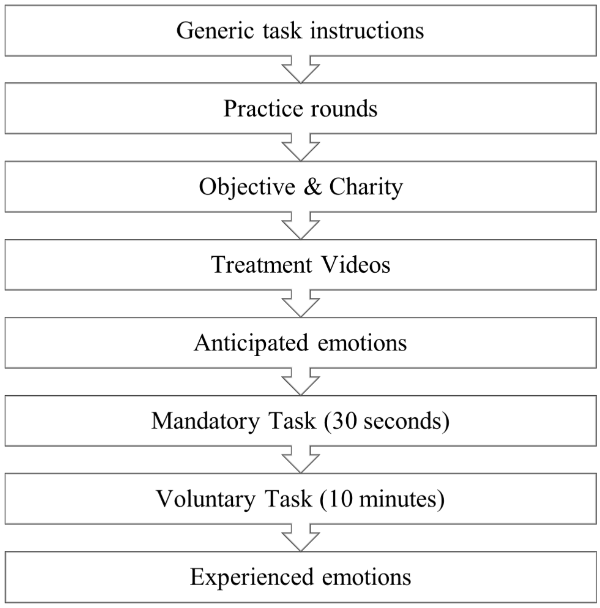

Figure 3.2.1: Experimental survey design

limit for each trial. The practice round did not contribute to the generation of donations for the environmental charity and had the sole purpose to familiarize participants with the task. After the trial period, participants were informed about the objective of the task “to score as many correct completions as fast as possible during 30 seconds” and informed that each correct completion would generate a donation of 2.5p on their behalf to our partnered environmental charity.[7]

After the instructions and prior to starting the mandatory part of the pro-environmental effort task, participants were shown one of four treatment videos. They were then asked to rate the sentiment of the video (as a manipulation check) and report their anticipated emotions ("how helping the environment would make them feel"), immediately followed by the thirty-second mandatory pro-environmental effort task. To conclude the mandatory part of the survey, participants reported perceived difficulty and enjoyability of the task. It is important to note that the financial reward (£0.40) for completing the survey was based entirely on the estimated time of 2-3 minutes required to complete the mandatory part only. Participants thus had no financial incentive (or perceived obligation) to complete the voluntary part of the survey.

At this point, participants were shown their Prolific completion code and required to verify their submission on Prolific.co. On the same page, they were notified about the possibility to complete the voluntary part of the questionnaire, in which they could generate an additional

[7] A short text provided information on Friend’s of the Earth and what the donation would contribute towards.

donation for Friends of the Earth. Participants were clearly informed that this part of the survey was entirely voluntary, would not be financially compensated, and that they could stop at any time. If they chose to continue, they were then shown the same real effort task which they could continue for up to 10 minutes (200 trials) or exit at any time via an 'exit button'.[8] Upon completion or exit of the task, participants reported their experienced emotions (how helping the environment made them feel), except for those who did not participate in the voluntary part of the survey.

Follow-up survey: Participants were asked to complete a follow-up survey 48 hours after completion of the main experiment. The follow-up survey followed the same structure, containing the same mandatory and voluntary pro-environmental effort task and measures of anticipated and experienced emotions, but excluded the treatment videos.

3.2.3 Sample statistics

Table 3.2.1 presents summary statistics for the socio-demographic characteristics of the sample collected in the baseline survey. Baseline socio-demographic characteristics are reported for the sample that completed the first experimental survey (N=2689).

[8]The donation generated from both the mandatory and voluntary parts were totalled and donated to Friends of the Earth after data collection had been completed. Moreover, participants were informed about the amount they had generated via direct message after each survey wave.

Table 3.2.1: Summary statistics

Variable	N	Mean	Std. Dev.	Min	Max
Female (%)	2696	.612	.487	0	1
Age (years)	2698	36.748	13.809	18	87
Life satisfaction (scale)	2698	6.113	2.07	0	10
Life worthwhile (scale)	2698	6.342	2.263	0	10
Altruistic Values (scale)	2698	5.43	1.425	-.5	7
Biospheric Values (scale)	2698	5.146	1.667	-1	7
Income					
Less than £10,000	2698	.099	.299	0	1
£10,000 - £20,000	2698	.16	.366	0	1
£20,000 - £30,000	2698	.216	.411	0	1
£30,000 - £40,000	2698	.181	.385	0	1
£40,000 - £50,000	2698	.12	.326	0	1
More than £50,000	2698	.224	.417	0	1
Highest Educational Qual.					
No school leaving qualification	2698	.01	.101	0	1
GCSEs or equivalent	2698	.115	.319	0	1
A-levels or equivalent	2698	.29	.454	0	1
Higher Education qualification	2698	.585	.493	0	1
Charitable Behaviour					
Never	2698	.235	.424	0	1
A few times a year	2698	.544	.498	0	1
About once a month (or more)	2698	.173	.378	0	1
About once a week (or more)	2698	.049	.215	0	1
Annual Donation Behaviour					
None at all	2698	.2	.4	0	1
Up to £50	2698	.454	.498	0	1
£51-£100	2698	.162	.368	0	1
£101-£300	2698	.115	.319	0	1
£301-£500	2698	.034	.182	0	1
£501-£1000	2698	.017	.131	0	1
Over £1000	2698	.017	.131	0	1

Note: Table displays the summary statistics of socio-demographic variables for participants of the main experimental survey (N = 2698)

Of the participants that completed the experimental survey, 61% were female (information on the participant's gender was not available for two participants) and the average age was 37 years. The level of altruistic values (M= 5.4) and biospheric values (M=5.1) was generally high in our sample. Over half of participants were educated to degree level (i.e., higher education qualification), while average household income was evenly distributed across the six income brackets. Charitable behaviour was relatively uncommon in our sample. About a quarter of participants indicated that they never donate or volunteer for charity, while approximately half of participants contribute once a year. The final quarter of participants said they donate or volunteer at least once a month or more frequently. Similarly, 20% of respondents indicated that they do not donate any money to charity, whereas 45% of participants donate up to £50 per year. Only about 7% of the sample give more than £300 per year.

Table 3.2.2: Summary statistics: Outcome variables by survey wave

	Experimental Survey			Follow-up Survey		
	Mean	Std. Dev	*N*	Mean	Std. Dev	*N*
Donation generated (£)	1.13	(1.63)	2,698	1.09	(1.71)	2,597
Participation in voluntary part (%)	0.47	(0.50)	2,698	0.40	(0.49)	2,597
Time worked for charity (min)	2.60	(3.61)	2,698	2.41	(3.66)	2,597
Share of correct submissions (%)	0.89	(0.16)	1,248	0.92	(0.13)	1,022

Note: Table displays the summary statistics of the main outcome variables in both the experimental survey and follow-up survey. 'Share of correct submissions' is only available for individuals who started/completed the voluntary pro-environmental effort task.

Table 3.2.2 presents the means and standard deviations of our main dependent variables in both the experimental and follow-up surveys. The average donation generated for Friends of the Earth, our primary measure of pro-environmental effort, was £1.13 in the main experimental survey and £1.09 in the follow-up survey. In the experimental survey, slightly less than half of all participants (47%) participated in the voluntary pro-environmental effort task, while this share decreased to 40% in the follow-up survey. Participants also spent slightly less time on the task in the follow-up survey (2.4 minutes) as opposed to the experimental survey (2.6 minutes), but marginally improved their ability, which is reflected by a higher share of correct submissions (92% vs 89%).

3.2.4 Attrition and balance

As pre-specified, we took several measures to minimise attrition across survey waves. First, participants were informed that they would be rewarded with an additional bonus payment of £1 if they completed all survey waves.[9] Moreover, participants received personalised reminder messages on the evening prior to each experimental survey and at 8pm on the day of the survey (if they had not yet completed the survey 8 hours after it was published on Prolific). As a result, attrition was low across waves. Of the 2,980 participants randomly assigned to a treatment condition, 282 (9.46%) did not complete Wave 2 and 101 (3.74% of the remaining participants) did not complete Wave 3. The attrition rates achieved in our study are substantially lower than previous research utilising the Prolific subject pool with a longitudinal design (Palan & Schitter, 2018). We find no evidence of differential attrition by treatment condition.

As previously discussed, our stratification and re-randomisation procedure was designed to achieve balance on pre-specified socio-demographic characteristics and baseline ability. In Appendix Table 3.A1 we check whether balance was maintained in our analysis sample (Waves 2 and 3) with respect to baseline characteristics. Columns (1) – (4) display the sample means for the baseline group and each treatment condition. Columns (5) to (7) display the differences in means between each treatment condition and the baseline (BL) group. We find that randomisation was successful and that after removing attritors, participants baseline characteristics in the four conditions are not statistically distinguishable from each other. We find that only biospheric values were slightly lower in the warm-glow group than in the baseline condition, significant at a 10% level.

3.3 Estimation

We estimate a series of linear regressions to explore the effect of each treatment message on pro-environmental effort. We estimate both cross-sectional and longitudinal models. For our baseline specification, we restrict the sample to observations from the first experimental survey (Wave 2) during which the treatment messages were administered. For the longitudinal analysis, we estimate a repeated measures linear mixed-effect model with a random effect for each individual. The statistical method for fitting the mixed-effect model is residual maximum likelihood. The baseline specification for the cross-sectional analysis, estimated by OLS, is presented below:

[9] As the original four-wave design was reduced to three waves, the bonus payment was adjusted to £0.75

$$Y_i = \alpha_1 + \beta_1 Warm_i + \beta_2 Cold_i + \beta_3 Norm_i + \gamma X_i + \varepsilon_i \tag{3.1}$$

where Y_i represents our outcome measure of pro-environmental behaviour (donation, quantity, quality, voluntary participation) of individual i. $Warm_i$, $Cold_i$ and $Norm_i$ are treatment indicators equal to one if the individual i was shown the warm glow, cold-prickle or social norm treatment video (the baseline condition serves as the reference category). As pre-specified, X_i is a vector of socio-demographic variables for individual i that are found to be unbalanced across groups. We thus only control for baseline biospheric values, which is unbalanced between the baseline condition and the warm glow condition. We estimate heteroscedasticity robust (Eicker-Huber-White) standard errors throughout the analysis. It is important to note that throughout our analysis, we estimate intention-to-treat (ITT) effects of our treatment messages on donation behaviour. While we set the video to autoplay and did not allow participants to skip the video (i.e., the continuation button appeared only after the video had finished playing), we are not able to guarantee that all participants actively watched the video and paid attention to its contents.

In a supplementary exploratory mediation analysis (Hayes, 2017) we examine the relationship between biospheric values, warm glow and pro-environmental behaviour. Specifically, we first explore whether the effect of biospheric and altruistic values on PEB is mediated by anticipated warm glow. Second, we examine whether experienced warm glow mediates the relationship between past and future pro-environmental effort. We follow a causal mediation method based on Imai, Keele, and Tingley (2010) and Imai, Keele, and Yamamoto (2010).[10] In its general form, the causal mediation analysis is based on the following two models:

$$MV_i = \alpha_2 + \beta_2 IV_i + \delta_2 X_i + \varepsilon_{i2} \tag{3.2}$$

$$Y_i = \alpha_3 + \beta_3 IV_i + \gamma MV_i + \delta_3 X_i + \varepsilon_{i3} \tag{3.3}$$

Where MV_i is the mediating variable and IV_i is the independent variable; Y_i is the outcome variable and X_i is a vector of control variables (excluding the mediator). In the first step (equation 3.2), we estimate the direct effect of the independent variable on the mediating variable (β_2). In the second step (equation 3.3), we estimate the effect of both the independent (β_3) and the mediating variable (γ) on the outcome variable Y_i. Following Hicks and Tingley

[10] The mediation analysis is implemented in Stata using the user-contributed command `medeff` (Hicks & Tingley, 2011).

(2011), we compute the Average Causal Mediated Effect (ACME) by taking the product of the coefficient on the independent variable (β_2) in equation (3.2) with the coefficient on the mediating variable (γ) in equation (3.3). The ACME is calculated by "simulating predicted values of the mediator or outcome variable, which we do not observe, and then calculating the appropriate quantities of interest" (Hicks & Tingley, 2011, p.4). Specifically, standard errors and confidence intervals for the ACME are estimated based on the quasi-Bayesian Monte Carlo approximation of King et al. (2000).

3.4 Results

In this section we investigate whether the treatment messages had an impact on anticipated and experienced emotions and donation behaviour, compared to the baseline group which received only basic information on climate change.

3.4.1 Manipulation checks

First, we assess how the treatment videos were perceived. To do so, we asked participants to rate the general sentiment of the video on a five-point Likert scale ranging from "extremely negative" to "extremely positive", immediately after viewing the video. Figure 3.4.1 shows the distribution of responses for each treatment condition in percentages.

A visual assessment of the response distributions indicates that the video sentiment was largely perceived as intended. Nearly all participants (90%) perceived the positively framed warm glow message to be either somewhat positive or extremely positive, while the negatively framed cold prickle message was perceived to be negative by most participants (60%). Although both the baseline condition (basic information message) and the social norm message did not explicitly address emotions in relation to PEB, they were perceived to portray a moderately positive rather than negative sentiment. The majority of participants indicated that they portrayed a somewhat positive or extremely positive sentiment. A simple ordered probit regression of perceived video sentiment on treatment indicators as in equation (3.1) confirms the visual assessment and shows that, on average, videos were perceived as intended (see Appendix Table 3.A2).

Next, we explore whether the treatment videos were successful in manipulating anticipated positive and negative affect, which was assessed after participants reported the perceived sentiment. For our analysis, we constructed measures of positive and negative affect based on 10 individual emotion items (see Section 3.2.2). Figure 3.4.2 depicts the average anticipated

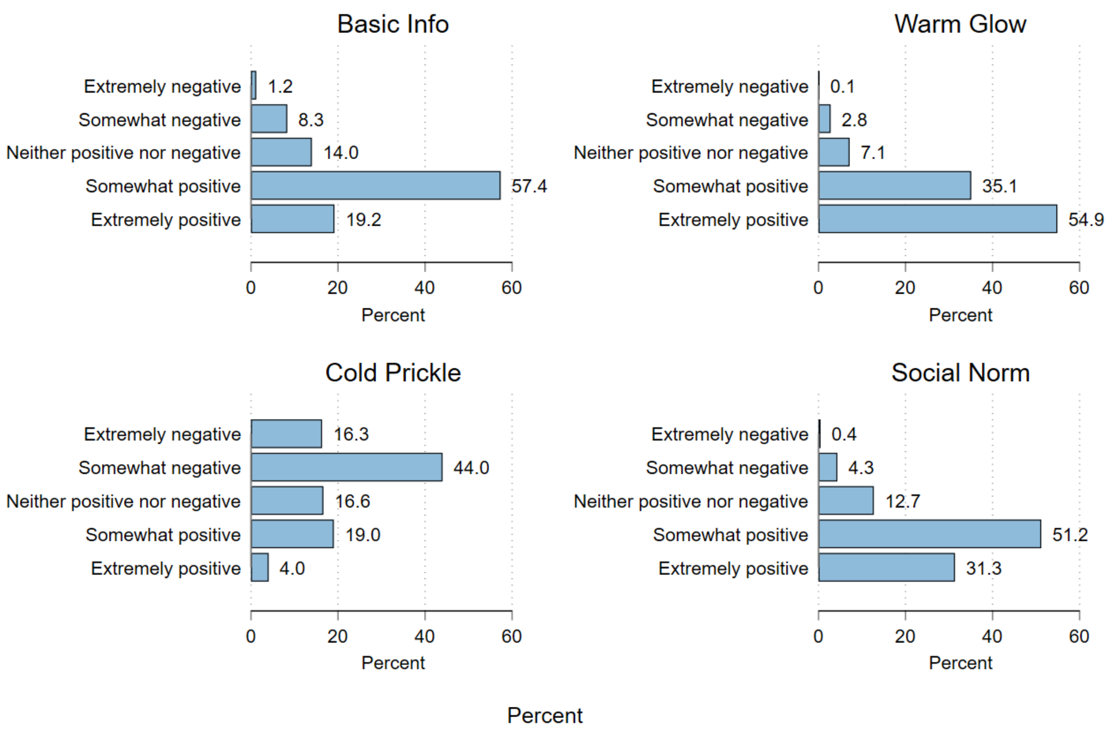

Figure 3.4.1: Self-reported perception of video sentiment by treatment group

positive and negative affect scores for each treatment condition with data from the main experimental survey (Wave 2) only. Table 3.4.1 presents the corresponding OLS estimates of equation (3.1) including biospheric values as a control variable.

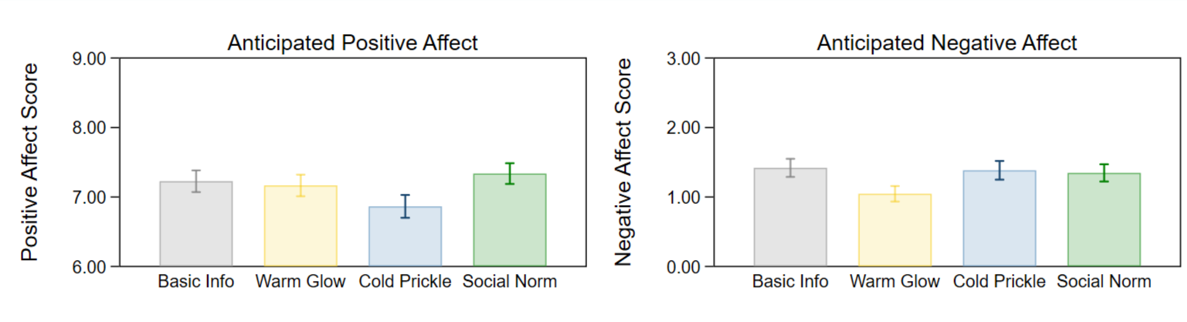

Figure 3.4.2: Anticipated positive and negative affect scores in Wave 2 by treatment condition

Note: Score range $0 - 10$, $N = 2,698$

In line with our hypotheses, we make the following three observations: (1) positive and negative affect are not significantly different in the social norm group, compared to the baseline group, (2) anticipated positive affect was significantly lower in the cold prickle group and (3) anticipated negative affect was significantly lower in the warm glow condition relative to the baseline condition, with both estimates being statistically significant at the 1% level. However, we do not find the hypothesised positive correlation between warm glow messaging and positive affect, nor a positive correlation between cold prickle messaging and negative affect. Although, as previously discussed, participants had an accurate perception of the sentiment portrayed by the videos, this analysis suggest that highlighting the emotional consequences of PEB only partially succeeded in experimentally manipulating anticipated affect.

Table 3.4.1: Direct effect of treatments on anticipated affect

	(1) Anticipated Positive Affect	(2) Anticipated Negative Affect
Warm Glow	0.031	-0.375***
	(0.098)	(0.087)
Cold Prickle	-0.352***	-0.036
	(0.101)	(0.096)
Social Norm	0.121	-0.073
	(0.097)	(0.092)
Biospheric Values	0.579***	-0.012
	(0.023)	(0.019)
Constant	4.218***	1.484***
	(0.143)	(0.115)
R^2	0.223	0.008
Observations	2698	2698

Note: OLS estimates of equation (3.1). The dependent variables are the *anticipated* positive and negative affect scores (ranging from 0 to 10), respectively. *Warm Glow*, *Cold Prickle* and *Social Norm* are treatment indicators identifying individuals randomly assigned to a respective condition. The omitted category is the *Basic Info* group. Robust standard errors in brackets.
* p < 0.1, ** p < 0.05, *** p < 0.01.

3.4.2 Main results

In this section, we explore whether the treatment messages had an immediate (short-term) effect on donation behaviour and experienced emotions. For this analysis, we restrict the sample to responses collected in the main experimental wave.

Main behavioural outcomes

Figure 3.4.3 displays the sample means for our primary outcome variable: mean donations generated in the voluntary part of the survey in GBP. Participants who did not participate in the voluntary part of the survey were coded as having generated a donation equal to zero.

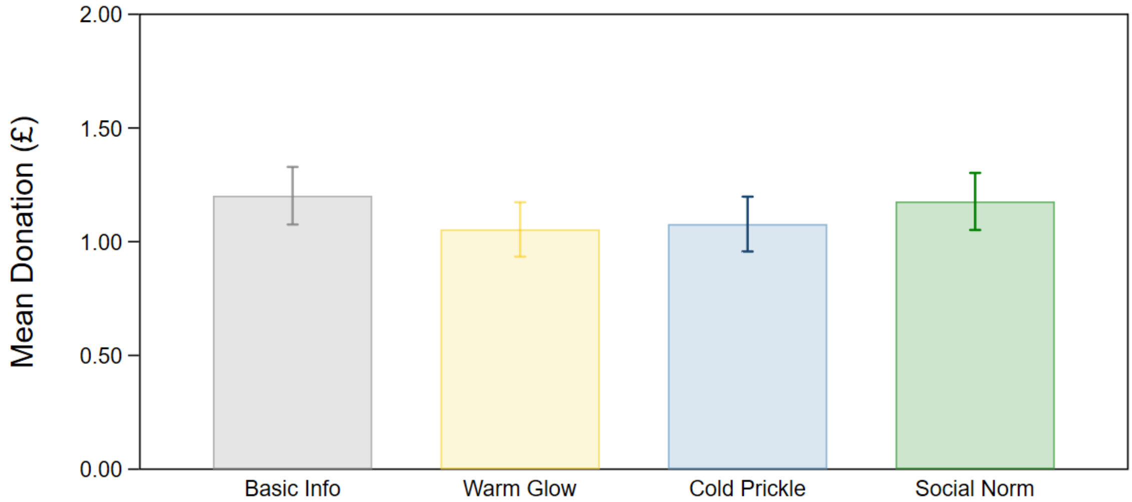

Figure 3.4.3: Mean donation across treatment conditions in main experimental wave

Note: Donation of participants who did not participate in the voluntary part were coded as zero, $N = 2,698$.

A visual assessment suggests that the average donation generated for the environmental charity was very similar across treatment conditions. Donations were highest in the baseline condition (£1.20), which provided only basic information on climate change and a call to action. In contrast to our expectations, mean donations were lowest amongst participants who viewed the warm glow treatment video. Average donations in the cold prickle condition were very similar to the amount generated in the warm glow treatment.

Table 3.4.2 presents the OLS estimates of equation (3.1) for each treatment condition relative to the baseline condition for all four outcome variables outlined in Section 3.2.2. The results indicate that, after controlling for biospheric values, none of the treatment conditions had a statistically significant effect on donation behaviour, relative to the baseline condition (column 1). Moreover, the treatment messages had no effect on participation in the voluntary part of the survey (column 2), time spent on the PEB-task (column 3) or the share of correct completions in the PEB-task (columns 4). Consistent with previous research (de Groot & Steg, 2008), biospheric value orientation (i.e., a one-unit increase in biospheric values on a 9-point scale) is found to be a significant predictor of all four measures of pro-environmental behaviour.

Table 3.4.2: Direct effect of treatments on donation behaviour

	(1) Donation (£)	(2) Voluntary Part	(3) Time Invested (Minutes)	(4) Effort Invested (Share Correct)
Warm Glow	-0.126	-0.015	-0.298	-0.009
	(0.088)	(0.027)	(0.195)	(0.013)
Cold Prickle	-0.122	-0.023	-0.261	-0.002
	(0.088)	(0.027)	(0.196)	(0.013)
Social Norm	-0.022	0.014	-0.097	0.014
	(0.090)	(0.027)	(0.199)	(0.012)
Biospheric Values	0.139***	0.047***	0.337***	-0.006**
	(0.017)	(0.005)	(0.037)	(0.003)
Constant	0.482***	0.233***	1.028***	0.919***
	(0.103)	(0.034)	(0.226)	(0.018)
R^2	0.022	0.025	0.026	0.006
Observations	2698	2698	2698	1248

Note: OLS estimates of equation (3.1). In the first column, the dependent variable is the donation amount generated in GBP(£). In the second column the dependent variable is an indicator identifying subjects that participated in the voluntary part of the survey. In the third column, the dependent variable is a continuous measure of the time spent completing the real-effort task. In the fourth column, the dependent variable is a measure of effort given by the share of correct submissions in the real-effort task. *Warm Glow*, *Cold Prickle* and *Social Norm* are treatment indicators identifying individuals randomly assigned to a respective condition. The omitted category is the *Basic Info* group. Robust standard errors in brackets.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Heterogeneity analysis

Altruistic and biospheric values have both been found to be important predictors of pro-environmental behaviour. We thus hypothesised that our message frames may have heterogeneous effects for people who hold higher or lower levels of baseline altruistic and biospheric values. We measured both types of values using a well-established 12-item values scale (de Groot & Steg, 2008). The scale is constructed based on responses to 12-items, asking respondents to indicate to what extent each statement serves as a guiding principle in their lives. The corresponding items form a reliable scale for both types of value orientation. To categorise individuals into high and low-values subgroups, we took the median split in our analysis sample. Above median individuals were considered as holding high levels of altruistic and biospheric values, whereas individuals below the median were labelled as holding low levels of values. It is important to note that the average levels of both altruistic and biospheric values were high (Median = 5.5, Min = -1, Max = 7). Individuals below the median thus do not necessarily represent "low" biospheric and altruistic individuals. However, the median split allows us to partition the sample into two equally sized groups.

We extend equation (3.1) to include a dummy variable identifying individuals with high altruistic values and its interaction with the treatment indicators. Means and corresponding 95% confidence intervals are visualised in Figure 3.4.4. For ease of interpretation and visualisation, we focus on cross-sectional results from the main experimental survey (Wave 2, as in Section 3.4.2).

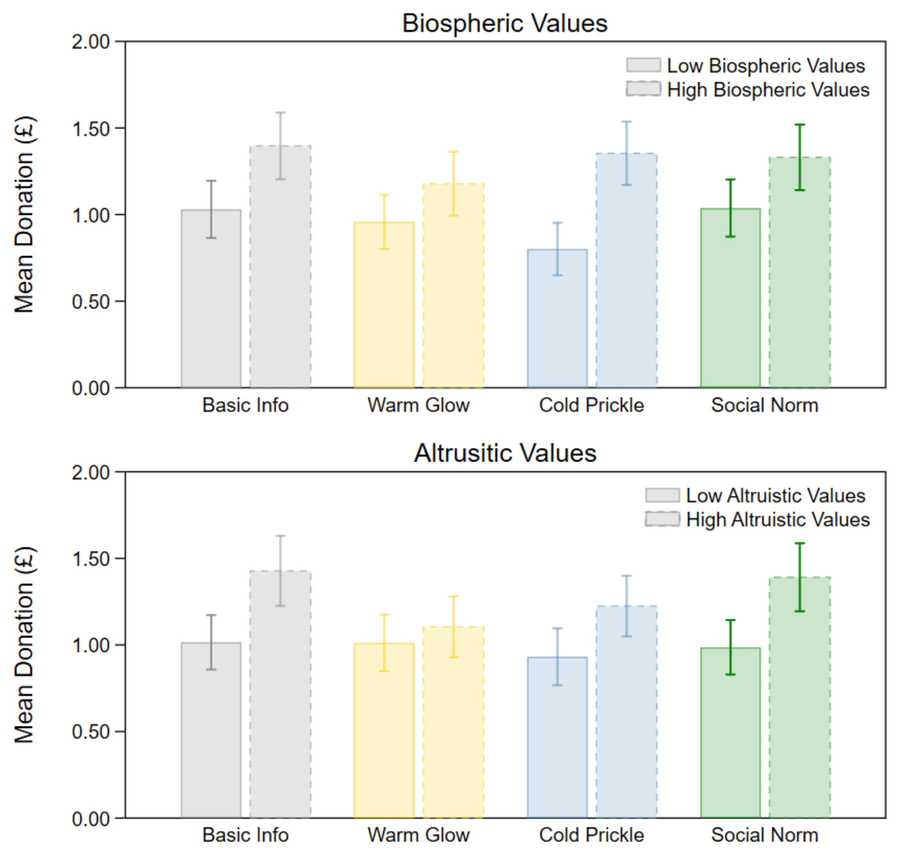

Figure 3.4.4: Mean donation across treatment conditions in Wave 2 by biospheric and altruistic values

Note: Bars with solid outlines display mean donations for the sub-sample of respondents with below median biospheric or altruistic values. Bars with dashed outlines display mean donations for the sub-sample of respondents with above median biospheric or altruistic values.

Two interesting findings emerge from this analysis. For subjects with below median biospheric values, the cold-prickle message significantly decreased donations compared to the baseline condition, significant at the 5% level. This finding suggests that highlighting the negative emotional consequences of failing to act pro-environmentally may be counterproductive to the objective of increasing PEB for people who are less inclined to hold biospheric values. Moreover, we observe that people with above median biospheric values donated less, on average, if they viewed the warm glow message, relative to the baseline condition. While this difference is not significant at meaningful levels (P=0.11), a similar and more pronounced pattern emerges for people who hold high levels of altruistic values and received the warm glow message, significant at the 5% level. Both findings suggest that appealing to warm glow benefits or the cold-prickle consequences may be counterproductive for certain individuals.

Experienced emotions

Finally, we explore whether emotions experienced during the PEB-task differed between treatment conditions for those individuals that participated in the voluntary task. Figure 3.4.5 displays average positive and negative affect scores based on responses to 10 emotion items assessed at the end of the experimental survey, after completion of the PEB-task. A visual assessment of the differences in means suggests that experienced positive affect was slightly lower in each of the three treatment conditions, relative to the control group. Average experienced negative affect is low across all four conditions, suggesting that most participants did not experience negative emotions whilst working for charity. Experienced negative affect is lowest in the warm glow condition, significantly lower than the baseline condition and the cold prickle condition at the 1% and 10% level, respectively.

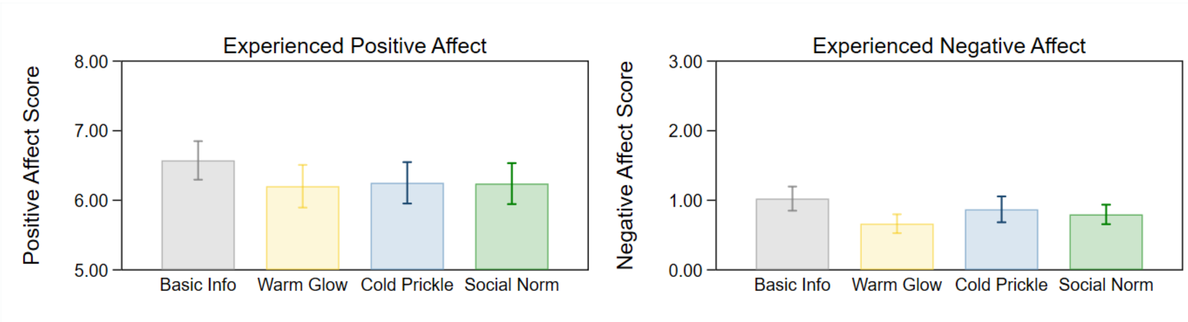

Figure 3.4.5: Experienced positive and negative affect scores in main experimental wave by treatment condition

Note: Score range $0 - 10$, $N = 1,212$.

Corresponding coefficient estimates from equation (3.1) are presented in Table 3.4.3. After controlling for biospheric values, both the cold prickle and social norm treatment messages significantly decreased average positive affect (at the 5% and 10% level, respectively) while experienced negative affect was lower in the warm glow and social norm groups, relative to the baseline condition (significant at the 1% and 5% level, respectively).

Table 3.4.3: Direct effect of treatments on experienced affect

	(1) Experienced Positive Affect	(2) Experienced Negative Affect
Warm Glow	-0.303	-0.360***
	(0.201)	(0.112)
Cold Prickle	-0.416**	-0.154
	(0.197)	(0.129)
Social Norm	-0.336*	-0.227**
	(0.199)	(0.114)
Biospheric Values	0.454***	-0.002
	(0.048)	(0.026)
Constant	4.112***	1.033***
	(0.286)	(0.162)
R^2	0.072	0.008
Observations	1212	1212

Note: OLS estimates of equation (3.1). The dependent variables are the *experienced* positive and negative affect scores (ranging from 0 to 10), respectively. *Warm Glow*, *Cold Prickle* and *Social Norm* are treatment indicators identifying individuals randomly assigned to a respective condition. The omitted category is the *Basic Info* group. Robust standard errors in brackets.
* p < 0.1, ** p < 0.05, *** p < 0.01.

As pre-specified, we hypothesised that anticipated positive emotions would partially mediate the relationship between the warm glow message and PEB. As we find neither a main effect of warm glow framing on donations, nor a statistically significant association between warm glow and experienced positive affect, we do not conduct formal mediation analysis.

3.4.3 Effects over time

In this section, we present results from the longitudinal analysis utilising the full data collected in both experimental surveys (main survey and follow-up). Estimates are obtained from repeated measures linear mixed-effect models via residual maximum likelihood estimation.

All models additionally control for baseline biospheric values. Figure 3.4.6 shows the mean donation generated across treatment conditions in both the main survey and the follow-up.

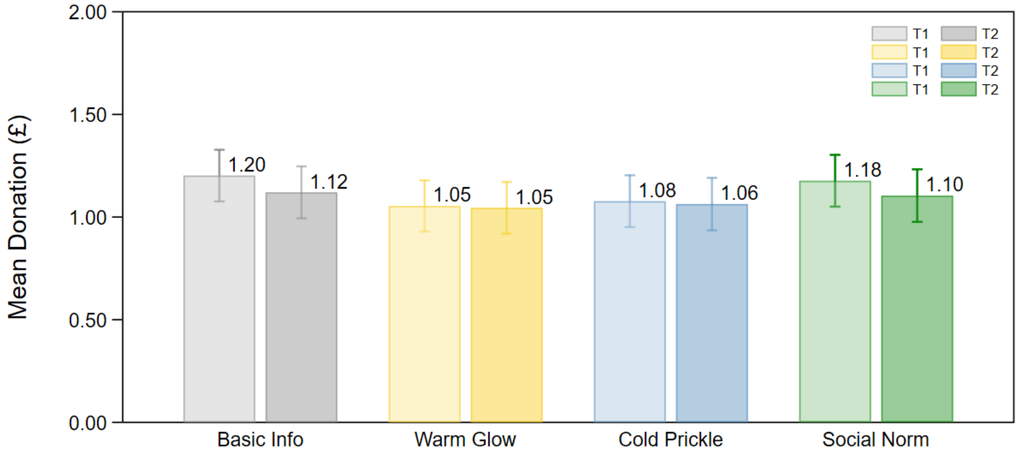

Figure 3.4.6: Mean donations in experimental survey (T1) and follow-up (T2) by treatment condition

Note: Donation of participants who did not participate in the voluntary part were coded as zero, $N = 5,295$.

We find that donation behaviour is largely unchanged over time and across treatment conditions. Donations slightly decreased in T2 for both the baseline condition and the social norm group, which performed best at T1. However, overall, none of the differences in means over time and across groups are statistically significant at the 10% level.

As in the first experimental survey (T1), participants were asked to rate their anticipated emotions (how would helping the environment make you feel), prior to completing the PEB-task in the mandatory part of the follow-up survey (T2). Figure 3.4.7 visualizes anticipated positive and negative affect at T1 and T2 by treatment condition.

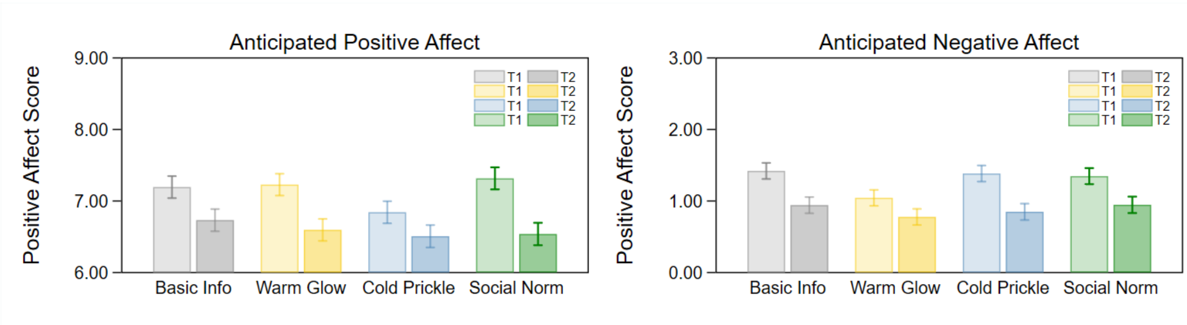

Figure 3.4.7: Anticipated positive and negative affect scores in experimental survey (T1) and follow-up (T2) by treatment condition

Note: Score range 0 − 10, $N = 5,295$.

A visual assessment suggests that both positive and negative affect scores were significantly lower at T2 than at T1 across all four conditions, highly statistically significant at the 1% level. This decrease in emotions is likely due to the absence of emotive priming and engagement with the topic of climate change via the treatment videos, which were displayed only at T1. Moreover, we observe that positive affect - which was lowest in the cold-prickle condition at T1 - is balanced across warm glow, cold-prickle and social norm groups at T2 but remains significantly lower in the cold-prickle and social norm groups compared to the baseline condition (significant at the 5% and 10% level, respectively). Negative affect remains lowest in the warm glow condition, significantly lower than in the baseline condition and the social norm group at T2 (significant at the 5% level).

Contrarily, we find that experienced emotions (Figure 3.4.8), assessed at the end of each survey and limited to those individuals that completed the voluntary part of the survey, remained largely unchanged between T1 and T2. In this regard, we only document a significant decrease in experienced negative affect in the cold prickle condition, significant at the 1% level. At T2, positive affect scores are significantly lower in the warm glow condition, relative to the baseline condition, significant at the 10% level.

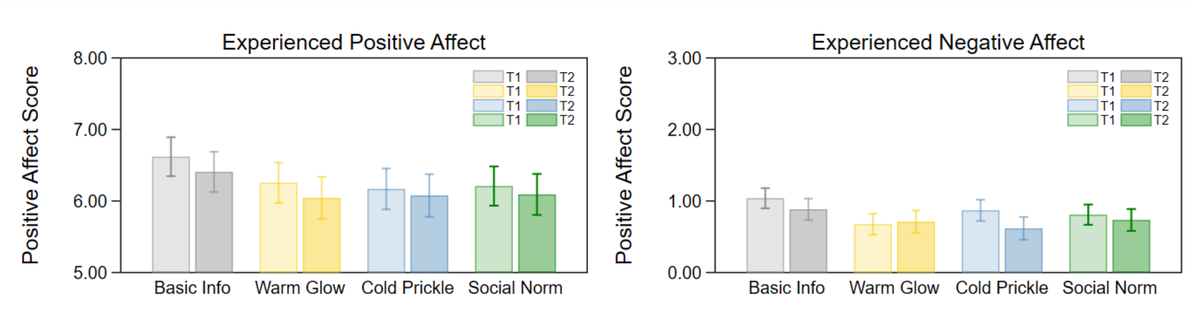

Figure 3.4.8: Experienced positive and negative affect scores in experimental survey (T1) and follow-up (T2) by treatment condition

Note: Score range $0 - 10$, $N = 2,221$.

In sum, the longitudinal analysis suggests that pro-environmental effort and experienced emotions are largely constant over time. While, anticipated emotions decrease across all four groups, likely due to the absence of a treatment stimulus, this does not appear to bring about significant changes in donations or experienced positive and negative affect.

3.4.4 Exploratory mediation analysis

In this section we present results from additional exploratory analyses on the interrelation between anticipated and experienced emotions and pro-environmental behaviour. First, we replicate a set of key findings from the existing literature, which have relied on pro-environmental intentions rather than incentive-compatible pro-environmental behaviour. van der Linden (2018) finds that anticipated warm glow predicts self-reported low-cost but not high-cost pro-environmental behaviour four weeks later. Moreover, follow-up research shows that the effect is driven by green warm glow (Jia & van der Linden, 2020). Similarly, Hartmann et al. (2017) explore to what extent the effect of altruistic value orientation on PEB is mediated by anticipated warm glow. All three studies highlight the importance of warm glow as an antecedent of PEB. Moreover, they provide support for the ‘impure altruism’ hypothesis (Andreoni, 1990), in that pro-social behaviour is partially motivated by benefits-to-self in the form of warm glow.

In Table 3.4.4 we present results from a simple correlational analysis (Panel A) and a mediation analysis (Panel B). We utilise data from Wave 2 (the first experimental survey). While our treatment messages had little to no effect on anticipated emotions (see Section 3.4.1), we nonetheless control for treatment assignment. Estimates are obtained from simple OLS regressions. Moreover, we present results from a causal mediation analysis in Panel B of the table (Imai, Keele, & Tingley, 2010; Imai, Keele, & Yamamoto, 2010).

Table 3.4.4: Mediation analysis: Anticipated warm glow and value orientation

	Donations			
	(1)	(2)	(3)	(4)
Panel A				
Anticipated Positive Affect (PA)	0.096***		0.047***	0.047***
	(0.014)		(0.017)	(0.017)
Biospheric Values (BV)		0.086***	0.066***	0.066***
		(0.022)	(0.025)	(0.025)
Altruistic Values (AV)		0.099***	0.085***	0.085***
		(0.024)	(0.028)	(0.028)
Constant	0.505***	0.213*	0.051	0.051
	(0.113)	(0.115)	(0.149)	(0.149)
R2	0.017	0.026	0.029	0.029
Observations	2698	2698	2698	2698
Panel B				
Independent Variable			*IV* = BV	*IV* = AV
Mediating Variable			*MV* = PA	*MV* = PA
ACME			0.020***	0.014***
Direct Effect (DE)			0.065***	0.084***
Total Effect (TE)			0.09***	0.10***
Percentage Mediated (%)			23.72	14.14

Note: Panel A presents estimates from a correlational analysis in which the dependent variable is the donation amount generated (£) in columns (1) to (4). Robust standard errors in brackets. Panel B presents estimates from a causal mediation analysis. ACME represents the Average Causal Mediation Effect.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Results indicate that anticipated positive affect is an important predictor of donations generated for an environmental charity (Column 1). A one-unit increase on the 11-point anticipated positive affect (APA) scale is associated with 9.6p increase in donations, on average. Column 2 shows that both biospheric and altruistic values are highly correlated with pro-environmental donations. A one-unit increase on the 9-point biospheric values (BV) altruistic values (AV) scale is associated with a respective increase of 8.6p and 9.9p in donations, on average. In columns (3) and (4) of Panel A, we observe that all parameter estimates decrease when both values and anticipated positive affect are included in the model, suggesting that the effect of value orientation may be partially mediated by anticipated warm glow (positive affect).

In Table 3.4.4, Panel B, we provide outputs from a causal mediation analysis. In both mediation models, the Average Causal Mediated Effect (ACME) is highly statistically significant, providing evidence of an indirect relationship between value orientation (the independent variable, IV), anticipated positive affect (the mediating variable, MV) and pro-environmental donations (the dependent variable, DV). The results suggest that approximately 24% of the effect of biospheric values on pro-environmental donations is mediated by anticipated warm glow from helping the environment (Column 3). Similarly, anticipated warm glow also mediates the effect of altruistic values on donations, however, to a slightly smaller extent: 14.14% of the total effect is mediated by anticipated positive affect.

While the term ‘warm glow’ was coined by economist James Andreoni (1989, 1990), a large literature in psychology has since shown that spending money on others produces a warm glow effect by improving happiness and well-being (e.g. Aknin, Wiwad, et al., 2018; Dunn et al., 2014, 2008). Moreover, this literature suggests that pro-social spending promotes and reinforces happiness, forming a positive feedback loop with behaviour and experienced warm glow (Aknin et al., 2012; Aknin, Van de Vondervoort, et al., 2018). With mounting evidence that warm glow can take both the roles of behavioural antecedent and consequence of pro-social behaviour, the same proposition has been made with respect to pro-environmental behaviour, which can be considered a type of pro-social behaviour where the benefactor is the environment (Brosch, 2021; Hartmann et al., 2017; Schneider et al., 2021; van der Linden, 2018). To explore the possibility of a positive feedback loop, and how to stimulate it, thus remains an important empirical question with implications for public policy. However, as our own analysis shows, it remains notoriously challenging to exogenously manipulate warm glow experiences. Nonetheless, our longitudinal design allows us to explore the possibility of a positive feedback loop with our incentive-compatible measure of PEB. Specifically, we test whether experienced warm glow at T1 reinforces PEB at T2. Figure 3.4.9 illustrates the indirect mediation model describing the relationship between donations at T1 and donations at T2, mediated by experienced warm glow at T1.

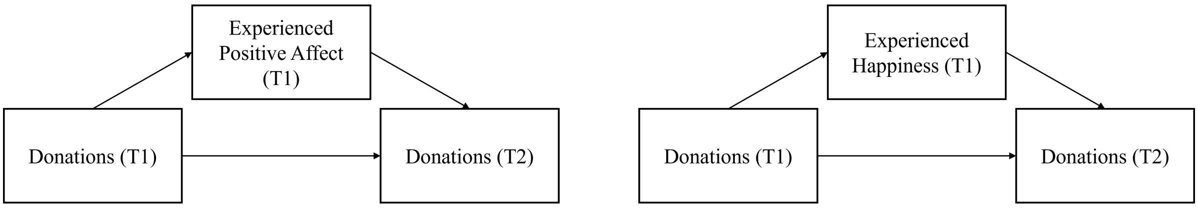

Figure 3.4.9: Indirect effect of donations at T1 on donations at T2 mediated by experienced PA (left) and happiness (right) at T1

In the left panel, we utilise our index of experienced positive affect (experienced warm glow) as our mediating variable. In the right panel, we follow the previous literature by employing a single-item measure of experienced happiness as the mediating variable (which was measured as one of our emotions items). It is important to note that the sample for this analysis is restricted to individuals who completed both surveys and also participated in the voluntary part of the survey at T1.[11] The regression outputs (Panel A) and formal mediation analysis outputs (Panel B) are presented in Table 3.4.5.

Column (1) of Panel A shows that pro-environmental donations at T1 are a weak predictor of experienced positive affect at T1, which does not reach statistical significance at meaningful levels. The estimates in Column (2) of Panel A suggest that pro-environmental donations at T2 are largely driven by donations at T1. Moreover, experienced positive affect at T1 has only a small effect on donations at T2, which is weakly statistically significant at the 10% level.

Formal mediation analysis (Column 2, Panel B) confirms that there is no statistically significant indirect relationship between past donations, experienced warm glow and future donations. The ratio of the ACME to the total effect suggests that only 0.41% is mediated by warm glow experiences. However, if we follow the existing literature from psychology and use experienced happiness, rather than an index of positive affect, we find a statistically significant mediating relationship (Column 4). The first stage regression shows that donations at T1 are associated with a statistically significant increase in experienced happiness at T1 (Column 3, Panel A). Moreover, both donations and experienced happiness at T1 have a statistically significant positive effect on donations at T2. While formal mediation analysis finds a statistically significant ACME of 0.008, the direct effect of past donations (at T1) on donations at T2 remains predominant. Only 1.17% of the total effect of past donations on future donations is mediated by experienced happiness, statistically significant at the 95% confidence level.

[11]Only participants who completed or exited the pro-environmental effort task provided measures of experienced emotions at the end of the survey.

Table 3.4.5: Mediation analysis: Experienced emotions and donations over time

	(1) Exp. Positive Affect (T1)	(2) Donations (T2)	(3) Exp. Happiness (T1)	(4) Donations (T2)
Panel A				
Donations (T1)	0.073	0.657***	0.148***	0.652***
	(0.048)	(0.030)	(0.045)	(0.030)
Exp. Positive Affect (T1)		0.035*		
		(0.018)		
Exp. Happiness (T1)				0.050***
				(0.019)
Constant	6.409***	0.113	6.980***	-0.015
	(0.192)	(0.151)	(0.182)	(0.165)
R2	0.005	0.303	0.015	0.304
Observations	1175	1175	1175	1175
Panel B				
Independent Variable		*IV* = D(T1)		*IV* = D(T1)
Mediating Variable		*MV* = PA(T1)		*MV* = HAP(T1)
ACME		0.003		0.008**
Direct Effect (DE)		0.66***		0.65***
Total Effect (TE)		0.66***		0.66***
Percentage Mediated (%)		0.41		1.17

Note: Panel A presents estimates from a correlational analysis. The dependent variables in columns (1) and (3) are experienced positive affect and experienced happiness at T1. The dependent variable in columsn (2) and (4) is the donation amount generated (£) at T2. Robust standard errors in brackets. Panel B presents estimates from a causal mediation analysis. ACME represents the Average Causal Mediation Effect.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The findings from this analysis suggest that experienced warm glow may not be as an important mediator as previously believed, at least among individuals who are already inclined to contribute to climate action. Much rather, donation behaviour appears to be relatively constant over time and only a small percentage is mediated by experienced happiness. When taking into account other emotions which may form warm glow experiences, proxied by an index of experienced positive affect, the mediating relationship is statistically insignificant. It is important to note that this analysis excludes individuals who made zero contribution to charity, thus limiting the sample to already highly motivated "green" participants.

3.5 Discussion and conclusion

The objective of this study was to explore the relative efficacy of different messaging appeals on pro-environmental behaviour in a controlled experimental setting. Participants were randomly assigned to view one of four treatment videos which increased the salience of either intrinsic or extrinsic motives to engage in pro-environmental behaviour or provided only basic information on climate change. The intrinsically framed treatment message directly appealed to the positive emotional reward (warm glow) derived from engaging in pro-environmental behaviour or the negative moral emotions (cold prickle) experienced as a consequence of failing to help the environment. The extrinsically framed treatment message increased the salience of an injunctive social norm describing the expected behaviour.

To measure PEB in an online experimental setting, we used an incentivised effort-donation paradigm, which allowed participants to generate a donation for an environmental charity. Moreover, we hypothesised that emotions would be an important mediating factor through which our treatment messages affect pro-environmental effort. We thus used self-report scales to measure both anticipated and experienced emotions derived from the cognitive appraisal of helping the environment. Specifically, we measured both anticipated and experienced positive affect (or warm glow) and negative affect (or cold prickle). To explore the persistency of our treatment interventions, we elicited pro-environmental effort with the same experimental task, two days after the first experimental survey.

Results indicate that pro-environmental action is largely constant across treatments and over time. Interestingly, we find that pro-environmental effort is highest in the baseline condition, which received only basic information on climate change. In contrast to our expectations, neither emotive appeals (warm glow and cold prickle) nor social norm messaging significantly increased donations relative to the baseline condition (basic information). Our findings thus diverge from recent field-experimental evidence, which showed that warm glow appeals perform significantly better than social norm or altruistic appeals in encouraging generic altruistic behaviour (Bergquist et al., 2020; List et al., 2021).

Nevertheless, it is important to note that absence of evidence (based on our non-significant results) does not imply evidence of absence. To test for the latter, we conduct a series of equivalence tests (Lakens et al., 2018). In equivalence testing, the researcher first decides on a smallest effect size of interest (SESOI or Δ), a value at which a treatment effect would be considered "meaningful". This value is used to determine a lower and upper equivalence bound (Δ_L and Δ_U) around the expected true effect size, which define an interval within which the treatment effect can be considered negligible. If differences at least as small as Δ_L or at least as large as Δ_U can be rejected, then the effect size can be considered trivially small or "statistically equivalent" (Lakens et al., 2018; List et al., 2011). As our primary outcome variable (donations) is obtained from a novel pro-environmental effort task, there exists no clear precedent, nor theoretical or practical boundaries, on which to base the SESOI. In such a case, a common approach is to utilise the minimum detectable effect size (MDE) obtained from ex-post power analysis as the SESOI (Lakens, 2017). For our primary outcome, we compute a MDE of £0.26 at 80% power and a 5% significance level, using the observations corresponding to our main cross-sectional analysis sample (Section 3.4.2). We round up this value to £0.30 which corresponds to 12 correctly completed trials (or approximately 30 seconds extra time spent on the task) and represents a small treatment effect (Cohen's Delta = 0.18). To test whether the treatment effects from our main analysis are equivalent to zero, we use the two one-sided tests (TOST) procedure (Schuirmann, 1987).[12] The results are presented in Appendix Table 3.A3 and suggest that, at a 5% significance level, the null hypothesis that effects are equal to or greater than the SESOI is rejected, thereby providing evidence of equivalence for each of the treatment effects.

Multiple factors may be responsible for the apparent null effects. Focussing first on the warm glow appeal, which highlighted the positive emotional reward of PEB, we find that the message was perceived as overwhelmingly positive but did not significantly affect positive affect or pro-environmental effort. One possible explanation is that people in the warm glow condition were already familiar with 'warm glow' experiences, and thus were unaffected by the treatment message. The call to action, which was held constant across all treatment

[12]The null hypothesis of equivalence ($\mathbf{H}_0^-$) of the treatment effect (β_t) within the equivalence bounds (Δ_L and Δ_U) can be tested using TOST: (1) $\mathbf{H}_{01}^- : \beta_t \geq \Delta_U$ (i.e., the treatment effect is equal to or larger than Δ_U) and $\mathbf{H}_{02}^- : \beta_t \leq \Delta_L$, (i.e., the treatment effect is smaller or equal to Δ_L). If both tests are *rejected*, and the classic null-hypothesis of significance, conducted in the main analysis, $\mathbf{H}_0^+ : |\beta_t| = 0$ (i.e., the treatment effect is statistically different from zero) is *not rejected*, we can conclude that the treatment effect is statistically equivalent to zero within the threshold determined by the SESOI. Equivalence tests are performed in Stata using commands from the user-contributed package `tostt` (Dinno, 2017).

conditions, may have been a sufficient stimulus to evoke warm glow emotions for participants in all four treatment groups. To that end, our supplementary analysis shows anticipated warm glow is an important mediating factor underlying the values-behaviour relationship, regardless of which treatment condition participants were assigned to.

With respect to cold prickle messaging, which increased the salience of negative emotions (e.g., guilt and shame), we find that the message was largely perceived as negative and significantly reduced anticipated and experienced positive affect but had no effect on donations. As with warm glow, if participants were already accustomed to warm glow experiences (i.e., they were already "glowing"), they may not have been affected by the negative framing. Nonetheless, cold-prickle messaging appears to erode the anticipated warm glow from helping the environment, which may have important implications for behaviour of specific subgroups of the population. Our heterogeneity analysis suggests that cold prickle framing significantly decreased pro-environmental effort amongst individuals with lower baseline biospheric values, which in turn is mediated by significant decreases in anticipated positive affect (or warm glow). Our results, thus, echo previous findings which have shown that guilt framing may not be an optimal strategy to nudge people with low environmental concern (Wonneberger, 2018).

Regarding the injunctive social norm message, we did not expect to find a significant effect on emotions but hypothesised that the message would significantly increase pro-environmental behaviour. However, we find a precisely estimated null effect on pro-environmental effort relative to the baseline condition. Several possible explanations may explain the absence of a treatment effect. One explanation may relate to the explicit wording of the social norm message. In a meta-analysis of social norm interventions, Bergquist et al. (2019) find that explicitly induced social norms (i.e., openly communicating people's behaviour or (dis)approvals) were less influential than implicitly induced social norms. Our social norm message both openly communicates the expected behaviour and additionally defines it as a social norm, which may have contributed to its ineffectiveness in changing behaviour.[13] Another possible explanation is that participants already had an accurate perception of the prevalent social norm described in the treatment message. Recent research shows that descriptive social norm messages are particularly effective if they correct people's misperceptions of the norm (Peter et al., 2021). Future research should attempt to measure baseline beliefs to further explore this hypothesis.

[13]Note that the wording of the social norm message was consciously selected to align the message text with those of the "warm glow" and "cold prickle" messages and maintain a consistent explainer-style format across all treatments.

A more general explanation for the null effects, which applies to all treatment conditions, is that people may have felt manipulated by the explicit wording of the messages. While we cannot empirically test this claim, some indication is provided by Gråd et al. (2021) who conducted a similar online experiment to ours.[14] Gråd et al. (2021) find that different types of nudges (including social norm and moral norm) did not increase donations to a charity for those subjects who felt that the nudges were an attempt to manipulate their donation behaviour. They conclude that, "if someone feels pressured or tricked into an action, the prosocial act might be less rewarding in terms of experienced warm glow" (Gråd et al., 2021, p.3). This, in turn, may induce information avoidance (Andreoni et al., 2017; Damgaard & Gravert, 2018) and implies that some people may be willing-to-pay to not receive the information if it entails negative welfare effects (Allcott & Kessler, 2019). In line with this notion, we find some indication that our treatment messages made engaging in pro-environmental behaviour less emotionally rewarding. Experienced warm glow (measured by positive affect) at the end of the experiment was lower in all three treatment conditions compared to the baseline condition, which provided no explicit "nudge". However, even if participants felt that the messages were an attempt to manipulate their behaviour, this does not appear to have had a negative impact on pro-environmental effort either.

Finally, we cannot entirely rule out alternative explanations associated with the design of the treatment intervention. Although the sentiment portrayed by both emotive treatment messages (warm glow and cold prickle) was perceived as intended, the videos may have not provided sufficient "stimulus" to create warm glow or cold prickle experiences and significantly shift emotions. Moreover, the baseline condition – although neutrally framed – was also generally perceived as positive, thus narrowing the scope in which to significantly influence positive emotions. A follow-up study, which lies beyond the financial scope of this thesis, could be used to gain additional insights, and test the plausibility of the previously discussed explanations for the apparent absence of treatment effects in our study. Specifically, a subset of participants from each treatment condition could be recontacted to complete a follow-up survey. Participants would rewatch the video and be asked to recall how they felt whilst watching the video during the main experiment. Participants would then be asked to rate the video based on its informational and emotive appeal, and indicate whether they perceived the video to be an attempt to manipulate their donation behaviour. More general questions

[14] Gråd et al. (2021) conducted an online experiment to explore whether three types of nudges (default nudge, social norm nudge and moral nudge) crowd out donations to a charity (UNICEF) and warm glow measured as subjects' happiness with the donation decision. Moreover, their experimental design manipulates the degree of transparency of the nudges by informing a subgroup of participants of the nudges that were used. The results suggest that all three types of nudges increased donations, driven by individuals who did not perceive the nudges to be manipulative. Moreover, self-reported happiness (or warm glow) was not affected by the interventions.

could be used to capture additional participant characteristics and beliefs. For instance, participants could be classified into "warm glow types" independent of the current study context, using a warm glow survey question as in Carpenter (2021).[15] Moreover, perceptions of pro-environmental social norms could be elicited. Responses from the survey could then be merged to the data from the main experiment to explore additional heterogeneity. Finally, the follow-up survey could measure intentions to engage in pro-environmental behaviour (following the treatment intervention) which would allow us to explore whether the absence of a treatment effect in the main experiment is due to the incentive-compatible nature of the pro-environmental effort task.

It is important to acknowledge certain limitations of this study. While our incentive-compatible experimental measure of pro-environmental behaviour – based on invested time and effort – presents a significant improvement over much of the previous research, it cannot be equated with actual every-day pro-environmental behaviours. Every day pro-environmental behaviours are likely to involve different costs and benefits and be subject to a range of additional contextual factors. Moreover, we acknowledge that the Prolific subject pool, from which we recruited our study sample, may not accurately represent the general population. Future research should continue to explore the efficacy of emotive appeals in the context of actual pro-environmental behaviours in real-world settings with representative samples. Finally, it is important to highlight the challenges involved with measuring emotions using self-report measures, which are vulnerable to measurement error. Subjects may pay little attention to the measurement items or consciously misreport their emotions. Future research should therefore attempt to measure emotional response using physiological and objective measures of emotions, to validate self-reports. For instance, sensor wristbands could be used to obtain physiological measures of emotional arousal (e.g., electrodermal skin conductance, heart rate variability) and facial-recognition software could be utilised to objectively measure emotional response and identify different discrete emotions.

In sum, the results from this study highlight the difficulty of exogenously manipulating people's intrinsic motivation (Kácha & Ruggeri, 2018). Although our supplementary analysis confirms the importance of anticipated warm glow as an antecedent to PEB, directly appealing to warm-glow motives did not significantly affect anticipated positive emotions or pro-environmental behaviour. Furthermore, neither negative emotive framing nor social norm messaging had a significant effect on PEB. Contrary to expectations, pro-environmental

[15] Carpenter (2021, p.560) categorises participants into high and low warm glow types based on the following survey question: "Think about the last time you gave to charity before today. What was most important to you, (i) the total amount given by everyone, (ii) the amount that you personally gave or (iii) some other aspect of giving?"

behaviour (as measured by our real-effort task) was surprisingly consistent across treatments and over time. To that end, we show that baseline donations are the strongest predictor of future pro-environmental behaviour and experienced warm glow does not significantly mediate this relationship amongst people that already contribute.

While our discussion outlines potential reasons for the apparent null effects, on a more practical note, our findings are likely not too far from the reality of many real-world communication efforts aimed at encouraging pro-environmental behaviour. Simple messages with subtle differences in framing are appealing to policymakers, as they are often seen as low-cost, easy to implement and nonintrusive. They lend themselves, in particular, to targeted communication campaigns – for example via social media channels – which will play an increasingly important role in shaping public opinion and behaviours. For instance, short videos, such as ours, could be used in YouTube Ad campaigns to directly appeal to relevant target groups. However, our findings show that simple informative and emotionally farmed messages, communicated using "explainer-style" videos, may not provide a large enough stimulus to influence pro-environmental effort or trigger a sufficiently large emotional response. Our findings, once again, highlight the difficulty of substantially shifting pro-environmental effort, which goes beyond mere intentions to act on climate change. To that end, more research is required to deepen our understanding of the intrinsic motivational basis of warm glow and how it can best be harnessed to encourage persistent behaviour change.

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Appendix

Appendix 3.A Additional tables and figures

Table 3.A1: Balance checks

Variable	(1) Baseline Info	(2) Warm Glow	(3) Cold Prickle	(4) Social Norm	(5) WG vs. BL	(6) CP vs. BL	(7) SN vs. BL
Female (%)	0.61	0.61	0.62	0.61	-0.00	0.02	0.00
	(0.49)	(0.49)	(0.48)	(0.49)	(0.03)	(0.03)	(0.03)
Age (Years)	37.23	37.00	36.56	36.20	-0.23	-0.67	-1.03
	(14.06)	(13.86)	(13.81)	(13.50)	(0.76)	(0.76)	(0.75)
Income	3.70	3.76	3.77	3.72	0.05	0.07	0.01
	(1.60)	(1.65)	(1.65)	(1.66)	(0.09)	(0.09)	(0.09)
Life Satisfaction	6.08	6.20	6.06	6.11	0.12	-0.02	0.04
	(2.12)	(2.03)	(2.11)	(2.02)	(0.11)	(0.12)	(0.11)
Education Level	3.42	3.43	3.48	3.47	0.01	0.06	0.05
	(0.75)	(0.75)	(0.72)	(0.71)	(0.04)	(0.04)	(0.04)
Chariable Behaviour	2.02	2.04	2.03	2.04	0.02	0.01	0.02
	(0.76)	(0.78)	(0.77)	(0.79)	(0.04)	(0.04)	(0.04)
Life Worthwile	6.31	6.44	6.26	6.36	0.13	-0.05	0.05
	(2.27)	(2.18)	(2.29)	(2.31)	(0.12)	(0.12)	(0.13)
Donation Behaviour	2.44	2.42	2.45	2.49	-0.02	0.01	0.04
	(1.26)	(1.23)	(1.30)	(1.33)	(0.07)	(0.07)	(0.07)
Altrusitic Values (scale)	5.44	5.38	5.47	5.43	-0.06	0.03	-0.01
	(1.41)	(1.42)	(1.43)	(1.44)	(0.08)	(0.08)	(0.08)
Biospheric Values (scale)	5.20	5.04	5.18	5.18	-0.16*	-0.02	-0.02
	(1.62)	(1.65)	(1.71)	(1.69)	(0.09)	(0.09)	(0.09)
Baseline Ability	2.34	2.33	2.35	2.37	-0.01	0.01	0.03
	(1.05)	(1.05)	(1.05)	(1.05)	(0.06)	(0.06)	(0.06)
Perceived Task Difficulty	3.34	3.42	3.44	3.36	0.08	0.09	0.02
	(1.08)	(1.09)	(1.09)	(1.10)	(0.06)	(0.06)	(0.06)
Perceived Task Enjoyability	3.05	3.06	3.07	3.05	0.02	0.02	0.01
	(1.20)	(1.19)	(1.18)	(1.22)	(0.06)	(0.06)	(0.07)
Observations	673	687	668	670	1,360	1,341	1,343

Note: This table presents balance checks of sample characteristics between the four treatment conditions. Columns (1) to (4) display the sample mean for each group, respecitvely. Columns (5) to (7) display the differences in the means of each treatment group compared to the 'Baseline Info', which was pre-registered as the comparison group. Significance stars on columns (5) to (7) indicate whether differences in means are statistically significant based on p-values obtained from two-sample t-tests.

Table 3.A2: Video sentiment check

	(1) Video Sentiment	
Baseline Info	0.000	(.)
Warm Glow	0.814***	(0.062)
Cold Prickle	-1.356***	(0.066)
Social Norm	0.301***	(0.056)
Observations	2698	

Note: Table presents estimates of a simple ordered probit regression. The dependent variable is a categorical (ordinal) variable capturing perceived video sentiment (measured on a 5-point scale ranging from "extremely negative" to "extremely positive"). The indepedent variables are indicators for each treatment condition (the baseline condition being the omitted category). Intercept cut-offs omitted from output. Robust standard errors in brackets

* p < 0.1, ** p < 0.05, *** p < 0.01.

Table 3.A3: Equivalence tests

			H_0^+		H_{01}^-		H_{02}^-				
	β	*s.e.*	t	p	$t1$	$p1$	$t2$	$p2$	Rej H_0^+	Rej H_0^-	Relevance
Warm Glow	-0.126	0.088	-1.430	0.153	4.835	0.000	1.974	0.024	No	Yes	Equivalence
Cold Prickle	-0.122	0.088	-1.393	0.164	4.810	0.000	2.024	0.022	No	Yes	Equivalence
Social Norm	-0.022	0.090	-0.248	0.804	3.572	0.000	3.077	0.001	No	Yes	Equivalence

Note: Table presents the test for difference (H_0^+) of the treatment effects β and two one-sided tests for the equivalence of means (H_0^-) within a symmetric equivalence interval ($\Delta = 0.3$), at a 5% significance level ($\alpha = 0.05$). Rej H_0^+ and Rej H_0^- report whether the test was rejected given pre-specified values of Δ and α. Relevance reports the conclusion drawn from the combined tests for difference and equivalence.

Chapter 4

Do carbon footprint labels promote climatarian diets? evidence from a large-scale field experiment

4.1 Introduction

There is growing awareness around the impact of dietary choices on climate change. It is estimated that the food system alone is responsible for 26%-34% of global greenhouse gas (GHG) emissions (Crippa et al., 2021; Poore & Nemecek, 2018), of which at least 15% are attributed to livestock farming (Gerber et al., 2013; Godfray et al., 2018).[1] Recent modelling suggests that even if fossil fuel emissions were immediately halted, current trends in global food systems would prevent the achievement of the IPCC's 1.5°C target and, by the end of the century, even threaten the attainment of the 2°C target (Clark et al., 2020). Studies also show that a shift towards diets with lower carbon footprints, so called climatarian diets, have the potential to meaningfully reduce carbon emissions and alleviate pressures on the environment (Kim et al., 2019; Poore & Nemecek, 2018; Willett et al., 2019).[2] With food related emissions being largely demand driven, policies that target food-demand management hold a significant

[1] Beyond climate impacts, ongoing agricultural expansion and intensification (in particular related to livestock production) have further far-reaching consequences for land degradation, deforestation and biodiversity loss (Daskalova et al., 2020; Williams et al., 2020).

[2] A climatarian diet is a diet that aims to reduce the carbon footprint from food consumption and mitigate climate change. Such a diet does not generally involve strict rules, but rather places a focus on mindfulness about food-related emissions. Climatarian diets may encompass a range of behaviours to lower one's carbon footprint from food consumption (e.g., reducing meat consumption, substituting to lower-impact meat alternatives, reducing food waste and packaging, or eating locally) and are consistent with a range of environmentally

greenhouse gas (GHG) mitigation potential (Bajželj et al., 2014; Reisch, 2021; Temme et al., 2020).

In a first best world, a Pigouvian intervention in the form of a widely discussed carbon tax targeting emissions from all products, or a more directed meat and dairy tax, can directly price in environmental externalities from livestock production and consumption and incentivise people to change their diets.[3] However, such taxes have faced fierce resistance in practice, due to a lack of popularity among voters. Taxes are also costly to design and implement, and simulations point out that particular carbon-based food taxes tend to be slightly regressive (García-Muros et al., 2017; Säll, 2018).

In response, academics and policy makers have turned to explore more subtle behavioural interventions, so called 'nudges', which constitute small changes in the choice architecture that can promote behavioural changes, without forbidding any options or significantly changing economic incentives (Thaler & Sunstein, 2009). Nudges have gained increasing popularity as an environmental policy instrument (for a review see Carlsson, Gravert, et al., 2021). In the realm of food choices, numerous studies have attempted to encourage more sustainable food choices through a range of behavioural interventions in different settings, including supermarkets and restaurants (Vandenbroele et al., 2020).[4] More recently, there has been a particular focus on interventions targeting meat and vegetarian consumption (Bianchi, Garnett, et al., 2018; Çoker & van der Linden, 2020). Interventions explored include changing the salience, order or positioning of meals in cafeteria and restaurant settings (Bacon et al., 2018; Garnett et al., 2020; Gravert & Kurz, 2019; Kurz, 2018). Overwhelmingly, the results suggest that these stimuli are associated with positive and significant increases in the share of vegetarian meals consumed. While the aforementioned behavioural interventions - targeting different aspects of the choice architecture (i.e., different ways in which choices can be presented) - have the ability to nudge consumers towards more sustainable meal choices, they usually fail to address the information asymmetries underlying the market failure associated with food production in the presence of environmental externalities. It remains unlikely that consumers will make socially optimal consumption decisions in a sustained manner if markets do not convey relevant information about the external costs of production and consumption (Moran, 2021).

motivated diets (including plant-based and flexitarian). In this paper, climatarian diet is defined as a diet that involves making consumption choices based on the carbon footprint of different foods.

[3]Experimental research shows that carbon taxation can reduce the carbon footprint of food consumption (Panzone et al., 2021; Panzone et al., 2018).

[4]A complementary experimental literature has explored the effect of behavioural interventions on healthier food choices. For recent reviews see Vecchio and Cavallo (2019) and Cadario and Chandon (2020).

In an effort to address the information asymmetry around the environmental impact of food, recent experimental studies have explored the efficacy of information and educational interventions in cafeteria (Jalil et al., 2020; Schwitzgebel et al., 2020) and supermarket settings (Elofsson et al., 2016; Kanay et al., 2021; Lanz et al., 2018). For instance, results from Jalil et al. (2020) show that a one-off lecture on the environmental impact of meat consumption increases vegetarian sales, however effects fade over time.[5] These findings suggest that consumers may fundamentally lack sufficient knowledge about the consequences of their food choices, or this information may not be salient at the time of purchase. Both issues may be addressed by providing clear environmental impact information in the form of carbon footprint labels. For instance, Camilleri et al. (2019) find that consumers misperceive the environmental impact of their diets and consistently tend to underestimate the carbon footprint of their food consumption. In addition, the study also shows that carbon labels can reduce such consumer misperceptions (Camilleri et al., 2019). Recent research corroborates that carbon footprint labels operate through improvements in consumer knowledge and are effective in reducing estimation biases for the carbon footprint of food (Panzone et al., 2020).

Only a small set of experimental studies have explored the impact of providing information on greenhouse gas emissions on actual meal choices via labels.[6] For instance, Osman and Thornton (2019) study hypothetical meal choices in a laboratory experiment and find that carbon labels can encourage sustainable meal choices compared to a condition where only basic meal information is provided. In a cafeteria setting, Spaargaren et al. (2013) find emissions reductions of less than 2% from a comprehensive climate labelling scheme on snack-like items. Slapø and Karevold (2019) find that traffic-light labels on warm dishes significantly reduced sales of meat dishes, but only during the first 20 days of the intervention at a university cafeteria in Norway. The study closest to ours is Brunner et al. (2018) who conducted an impact evaluation of the introduction of a carbon label at one university restaurant using a before-after intervention design. They find that sales of high-emission meat dishes decreased by 2.4 percentage points and low-emission meat dishes increased by 5.6 percentage points, resulting in an overall decrease in emissions of 3.6%. Although the aforementioned studies

[5]For a broader review of interventions targeting demand for meat including information provision, see Bianchi, Dorsel, et al. (2018).

[6]There are a considerable number of empirical studies on consumer response to carbon footprint and environmental sustainability food labels that come from hypothetical surveys or stated preference methods (see Rondoni and Grasso (2021) and Potter et al. (2021) for recent reviews; see Edenbrandt and Lagerkvist (2021) and Carlsson, Kataria, et al. (2021) for recent applications). Though this body of work is informative, the literature review in this chapter focuses on the relatively small number of field experimental studies on this topic, as these are more suitable for assessing the causal links between carbon food labels and actual food choices. Finally, a separate literature provides field experimental evidence on the effect of fair trade labelling on consumers' ethical food choices (e.g. Hainmueller et al., 2015).

were conducted in a field setting, all three studies utilise data from only a single restaurant with a limited number of purchase decisions.

Our study contributes to this literature by experimentally assessing the causal effect of carbon footprint labelling on individual meal choices in a university cafeteria setting using a large-scale field experiment. The study allows us to explicitly explore whether carbon footprint labels can induce more climatarian food choices and simultaneously quantify potential emissions reductions that can be attained from such changes in food consumption patterns. The experiment was conducted in partnership with five college cafeterias catering to students and staff at the University of Cambridge between October 2019 and March 2020. Carbon footprint labels were introduced at three of the five cafeterias on all cafeteria main meals served during an intervention period, while two cafeterias served as our control. We collected baseline (pre-treatment) meal choice data as well as a post-intervention follow-up exit survey data. The final dataset consists of nearly 85,000 individual dining decisions made by 2,682 individuals.

The present paper makes multiple contributions to the literature. First, our experimental field setting allows us to observe actual food choices in a real-world setting, while previous literature primarily relied on laboratory experiments or vignette studies, which do not accurately represent a real food choice setting. Second, our experimental design improves substantially on previous comparable field studies by implementing a difference-in-differences (DID) identification strategy with a significantly larger dataset. In contrast to previous studies, the availability of both treatment and control data, as well as baseline data, allows a causal interpretation of the results. Third, access to individual-level data enables us to track purchase decisions of the same individuals over time, allowing us to control for unobserved preference heterogeneity. Finally, our study is the first to provide insights into potential mechanisms and heterogeneous effects of carbon footprint labels by linking food purchase data with exit survey data collected at the treatment cafeterias after the intervention had been completed.

Our results indicate that carbon footprint labels have statistically significant effects on food choices. We find a significant substitution pattern between high and mid-carbon impact meals of approximately 2.7 percentage points. The reduction in high-carbon footprint meals is primarily driven by decreased sales of orange and red-labelled meat dishes, while the choice probability for yellow labelled vegan, vegetarian and fish dishes increased. Sales of low-carbon meals appear to be unaffected by labelling, on average. We find further evidence that the effect of labels on meal choices differs by pre-intervention preferences. Those individuals who followed a pre-dominantly high-carbon footprint diet in the pre-intervention period were most likely to reduce their consumption of high-carbon meals and increase mid-carbon meal purchases. Drawing on exit-survey data, we document that the information provided by the labels was perceived as trustworthy, useful and easy to understand. A supplementary analysis suggests that labels had a larger effect the happier they made customers feel about their food choices. With respect to emissions reduction, our estimates suggest that the carbon footprint labels caused a statistically significant average reduction of 27g CO_2 per 100g serving, corresponding to a 4.3% decrease in emissions.

4.2 Methods and data

4.2.1 Experimental design

We conducted a field experiment of carbon footprint labels on meals at five university cafeterias.[7] Each cafeteria was located within a University of Cambridge College, comparable to a hall of residence, which caters to its own (in residence) student population, academics and staff that are members of the College as well as a small number of guests. Our study focuses on the student populations of these five colleges, which range from approximately 500 to 1100 students. All colleges in our sample host both undergraduates and postgraduates as well as students from any academic discipline.

All cafeterias offered lunch and dinner services on weekdays, whereas three cafeterias were also open on weekends. The menu compositions in each cafeteria were planned by the cafeteria chefs before the start of each academic term and followed a pre-defined menu rotation. The menus were designed to cater towards different tastes and dietary preferences serving a variety of vegan, vegetarian, fish and meat dishes. The exact menu composition and the number of dishes available varied from day-to-day but generally included at least one vegan/vegetarian meal and a combination of fish and meat dishes. One cafeteria did not serve ruminant meat (beef and lamb). We thus take specific care to control for time-varying availability of different meal alternatives in our econometric models. Only small changes were made to the menus between academic terms. This feature uniquely benefits our identification, as diners faced recurring choice sets every four to six weeks throughout the experiment. Menu compositions for treatment and control cafeterias are shown in Table 4.A1 in the Appendix.

For all cafeterias, we obtained individual-level meals sales data, which were recorded via electronic sales registers. Whilst cash and/or credit card payments were generally accepted

[7] Ethical approval for the experiment was granted by the Department of Land Economy Ethics Research Committee.

for guests, college members could conveniently pay by swiping their university ID cards, which is the most common form of payment. Meal purchases made by college members were identified with an anonymous identifier, which allows us to track their meal choices over time. Alternatively, students have the possibility to eat out, order takeaway or cook their own food. However, the majority of undergraduate students do not have fully equipped kitchens in their student accommodation, which makes dining at the cafeteria a popular option. It is important to note that students were unlikely to switch between cafeterias in our study due to the college cafeteria system. Each college only allows its own members to conveniently dine at the college cafeteria, offering subsidised rates and automated purchases for student members via their ID cards. While it is possible to dine at other colleges upon invitation, this only happens occasionally, as students generally form a strong social network within their own college.

The experiment took place over the course of two academic terms, running from 7th October to 8th December and January 13th to March 15th. While the academic year consists of three terms - two teaching terms and one exam term - the university requires all students to be physically present in Cambridge during the first two terms in which our experiment was conducted, thus allowing us to observe meal choices of a consistent sample of students. Moreover, the study period covered both autumn and winter months during which weather patterns were relatively stable and unlikely to confound our results. The first academic term, as well as the first two weeks of the second term served as our baseline data collection period at all participating cafeterias. In total, the baseline period covered 9 weeks. At three treatment cafeterias, carbon footprint labels were introduced on Monday 27th January 2020 and displayed throughout the 7-week intervention period at lunch and dinner services for all cafeteria main meals.[8] The remaining two cafeterias served as a control group, which displayed no additional carbon footprint information.

4.2.2 Carbon footprint calculations

We calculated the carbon footprint of cafeteria recipes using life-cycle assessment (LCA) values from published systematic reviews (Clune et al., 2017; Hilborn et al., 2018; Poore & Nemecek, 2018). The system boundary used for the calculations was cradle-to-retail, in line

[8] Note that one cafeteria introduced the carbon footprint labels on Tuesday 28th January. The experiment was originally designed to run throughout the entire academic year and end after the third academic term (mid-June 2020). Labels were thus introduced in the third week of the second term in order to balance the length of the baseline and intervention period and allow all students to have returned to their term-time accommodation after the Christmas break. Unfortunately, all cafeterias were forced to close by 24th March due to the COVID-19 national lockdown, which somewhat shortened the intervention period.

with Poore and Nemecek's dataset. This covered greenhouse gas (GHG) emissions from farm, processing, packaging, transport, supply-chain storage and pre-consumption losses. GHG emissions arising from final-mile delivery, cooking, on-site storage and consumer losses were not included in the calculations. As per standard carbon footprint reporting, GHG emissions were expressed in kilograms of carbon dioxide equivalents.

LCA values are given per kilogram of food, requiring the standardisation of recipe ingredients for matching. All recipe units were converted to kilograms. Ingredients given in volumes were converted to kilograms using average density estimates from the online site aqua-calc.[9] Ingredients with discrete units (e.g. one apple) were converted to kilograms using average weights from three online UK supermarkets (Asda, Sainsbury's and Tesco).

LCA values were primarily taken from Poore and Nemecek's dataset (Poore & Nemecek, 2018). Simple ingredients in the recipes database (e.g. 500g chicken) were matched, at first instance, directly to LCA values from Poore and Nemecek's dataset. Where a direct match was not possible, the food item was matched to an available parent class LCA, created using food groups (for example, "Blackberries" matched to the LCA group "Berries"). Or, if a parent class was unavailable, then a substitution was made based on a similar production system, for example spinach was substituted with lettuce. When neither a parent class nor substitution was available, other datasets were used to fill in the gaps. Where possible, an attempt was made to harmonise the system boundary of these additional LCAs sourced from other datasets to the cradle-to-retail boundary. For example, average processing, packaging, transport and retail impacts from Poore and Nemecek were added to cradle-to-farmgate LCAs sourced from other datasets. Complex ingredients (those made of more than one simple ingredient) were broken down into their sub-ingredients based on manufacturer recipes, before matching to LCA values occurred. Manufacturer recipes do not always disclose the proportion of sub-ingredients. Therefore, proxy recipes from online recipe sources (e.g. BBC Good Food) were used to estimate the proportion of sub-ingredients in some complex ingredients.

The GHG emissions of food vary considerably by the location of production. We therefore used European LCA values from Poore and Nemecek for food items that can be produced in Europe, and British LCA values for all meat and dairy products. For items which are imported to Europe (e.g. bananas, cocoa), global average LCA values were used.

Once all LCA matching had occurred, the LCA values for each ingredient in the recipe were summed to give the final carbon footprint estimate for each recipe. Following the above

[9] https://www.aqua-calc.com/

methodology,[10] we calculated individual carbon footprints for approximately 500 unique cafeteria main meals based on detailed recipes provided by the cafeteria chefs.[11]

Appendix 4.E provides a rough illustration of the procedure. Figure 4.E1 depicts a typical recipe in the standard format provided by the cafeteria chefs. Note that the sample recipe contains two complex ingredients (burger buns and burger relish) which needed to be broken down into their sub-ingredients. Figure 4.E2 shows the same recipe in the data processing stage, after which it has been broken down into its individual component ingredients, converted into 'kilogram per serving' units and LCA values (per kilogram) have been matched. At this stage, the footprint for each ingredient (`ghg`) is calculated by multiplying the quantity (`qty`) field with the LCA value (`ghg_emis_pp`). In the following step, the quantity and footprint values are summed for each recipe so that they represent portion size and emissions per portion. To allow for comparisons between differently sized meals, the "per portion" values are then used to calculate the carbon footprint per 100 gram serving. A random subsample of 25 meals (five from each label-colour category) is shown in Appendix Figure 4.E3.

4.2.3 Intervention

To inform the label design, we conducted an extensive literature review to further inform important design elements of the carbon footprint label used for our study purposes. This literature has identified a combination of traffic-light design with a scale that puts information into context as the most comprehensible and more frequently trusted label design (e.g. Feucht & Zander, 2018; Meyerding et al., 2019; Muller et al., 2019; Panzone et al., 2020; Spaargaren et al., 2013; Thøgersen & Nielsen, 2016). We employed two UK-based graphic designers to create a set of label designs for the experiment and conducted an online survey with a small student sample (N = 93) from UK universities, recruited via Prolific Academic, to validate the most promising design. Survey participants were shown ten pre-selected designs and were asked to rate each label design across the following dimensions:

- **Information provided** - How much information does the label convey?
- **Degree of comprehensibility** - How easy is it to understand the information provided?
- **Visual appeal** - Is the label visually appealing?
- **Emotional appeal** - Does the label appeal to your emotions?

[10] We followed the methodology developed by Foodsteps Ltd., a UK-based sustainability consultancy in the food sector, who kindly provided their expertise to support the carbon footprint calculations.

[11] Recipe information was only available for treatment cafeteria dishes. However, for analysis purposes, we used our database of 500 unique meals to impute carbon footprint estimates for comparable dishes served in the control cafeterias. Many of the same dishes featured on both treatment and control cafeteria menus.

- **Appropriateness for cafeteria setting** - How suitable is the label to be displayed alongside cafeteria meals?

In addition, participants could provide additional comments via free-form survey questions and were asked to pick their three preferred label designs at the end of the survey. The survey findings reveal that students had a clear preference for more information (rather than less). For instance, some participants indicated that the information on the exact carbon footprint should be displayed, whereas others preferred a numeric scale with multiple cut-offs. In conclusion, a combination of both design elements was deemed the most suitable. Participants also had clear preferences for normative guidance. Labels which included either a traffic-light coloured scale or "good for climate" – "bad for climate" labelling consistently scored highest with respect to emotional appeal. A dial shaped design was considered the most visually appealing and appropriate for a cafeteria setting. In sum, the results suggest that the most preferred design elements included a differentiated scale providing both numerical and normative guidance (i.e., numerical cut-offs and traffic-light colours) presented using a dial-shaped layout. Drawing on these insights, the final label design was developed which is shown in Figure 4.2.1.

Figure 4.2.1: Carbon footprint label design

The label depicts the carbon footprint (CO_2 equivalent) per 100g serving of each meal, combined with a traffic-light coloured scheme and a numerical scale. For means of comparability across meals and cafeterias, the numerical value of the footprint was displayed in CO_2 per 100g serving. The numerical cut-offs for each label colour category were determined by splitting the entire sample of unique cafeteria meals (N=419) into quintiles based on their estimated carbon footprint. The quintile cut-offs were rounded to the nearest 50. The 20% of meals with the lowest carbon footprints had a footprint less than 150g CO_2 per 100g, whilst the 20% of meals with the highest footprints had a footprint greater than 800g CO_2 per 100g. The carbon footprint information displayed and relative placement of meals within the scale are thereby representative of a typical cafeteria meal.

Note that we had also considered alternative approaches to determining the scale cut-offs. For instance, the relative impact of different foods could be determined based on a daily personal GHG emissions budget (or "allowance") for food consumption. A recent WWF report computes an individual budget of 4.09kg CO_2e/day for the diet of UK individuals necessary to achieve carbon emission reductions targets by 2030 (Kramer et al., 2017), a reduction of 44% relative to 1990 levels. While this approach is appealing, as it draws on objectively determined carbon budgets and is comparable to calorie and nutrition labelling, which often displays information as a percentage of a daily allowance, it also has certain limitations. First, the carbon budget calculations are based on several crude assumptions, which may be inaccurate and inapplicable to the study population. Moreover, determining the relative impact of a meal would require further assumptions, such as the share of the budget available for a given cafeteria service. Students consume at most two cafeteria main meals per day, but make numerous additional food choices throughout the day, outside of the cafeteria. For these reasons, we concluded that a budget approach would be challenging to reconcile with a cafeteria choice setting. Consequently, we decided to base the label scale on the information available to us, which is representative of a typical cafeteria meal.

The final label design capitalises on the detailed footprint calculations, discussed in the previous section, to provide a nuanced and informative picture of the full range of carbon footprints of different types of foods, without being overly complicated to read and interpret.[12]

[12] For instance, a three-category label was considered as it may have been easier for customers to differentiate between. However, it may also have been perceived as less trustworthy and reliable, due to the lack of detail. Especially in a university setting, students are familiar with interpreting complex information, and we therefore believed that students would prefer a more detailed label, which was confirmed by our focus group surveys.

Figure 4.2.2: Experimental setting

At all three cafeterias, the labels were displayed in the servery directly above the cafeteria meals during lunch and dinner. The implementation was carried out by the cafeteria staff who received basic training and instructions to inform students about the labelling initiative, if asked. Importantly, this information did not reveal the study purpose, but simply informed students about the new labelling initiative. Responsibilities of the cafeteria staff included putting up and taking down the labels, collecting daily menu sheets and noting if any dishes had run out or been replaced, or if there were any other deviations from the planned menus. The implementation was monitored by the research team and spot checks were conducted at each cafeteria on three days per week. See Figure 4.2.2 for a picture of the experimental setting in one of the treatment cafeterias.

4.2.4 Data

We rely on individual-level sales data obtained from the cafeterias' Point of Sale providers. Sales data cover the entire experimental period (16 weeks) including the 9-week baseline period and the 7-week intervention period. While all cafeterias distinguished between sales to college members, staff and guests and applied different pricing regimes accordingly, only sales to student members could be effectively identified and tracked over the entire study period. We thus focus our analysis on college student members whose food choices could be associated with individual diners via their university IDs.[13] Finally, we limit the analysis to cafeteria main meals only (excluding sides, desserts and salads), as this is the primary focus of our study. We acknowledge that a complete analysis of the carbon footprint of meal choices would incorporate all components of a meal to fully capture any instances of behavioural compensation. However, sales of sides, desserts and salads are recorded in our sales data using generic identifiers which do not allow us to accurately identify which additional items were purchased.

Daily menu sheets were collected for the entire experimental period to track any deviations from the planned menu. Menus were merged with the sales data in order to identify which meal alternatives (i.e., choice sets) were available at a given service and which meal option was chosen by each individual. Sales coding in two treatment cafeterias and one control cafeteria allow us to observe the exact dishes chosen. In the third treatment cafeteria, sales coding does not distinguish between vegan or vegetarian sales, thus allowing us to only observe the exact meal choices for a subset of observations where either a vegan or vegetarian meal was available. In the second control cafeteria, the sales coding is limited to vegan/vegetarian or fish/meat. Sales data from this cafeteria can, therefore, only be used in a binary choice model with two aggregate alternatives. Prior to our analysis, choice sets were adjusted to reflect the alternatives available at any given time. For instance, if an alternative had run out after a certain time, it was removed from all subsequent choice sets, or adjusted, if a replacement was made available. The final dataset consists of 84,307 individual purchase decisions made by 2,682 individuals. Figure 4.2.3 utilises the full dataset to plot the total sales of cafeteria main meals aggregated on a weekly basis.

[13]Note that we excluded any individual diners who bought more than one meal at a given cafeteria service, as we are not able to determine whether additional meals were purchased for themselves or other people.

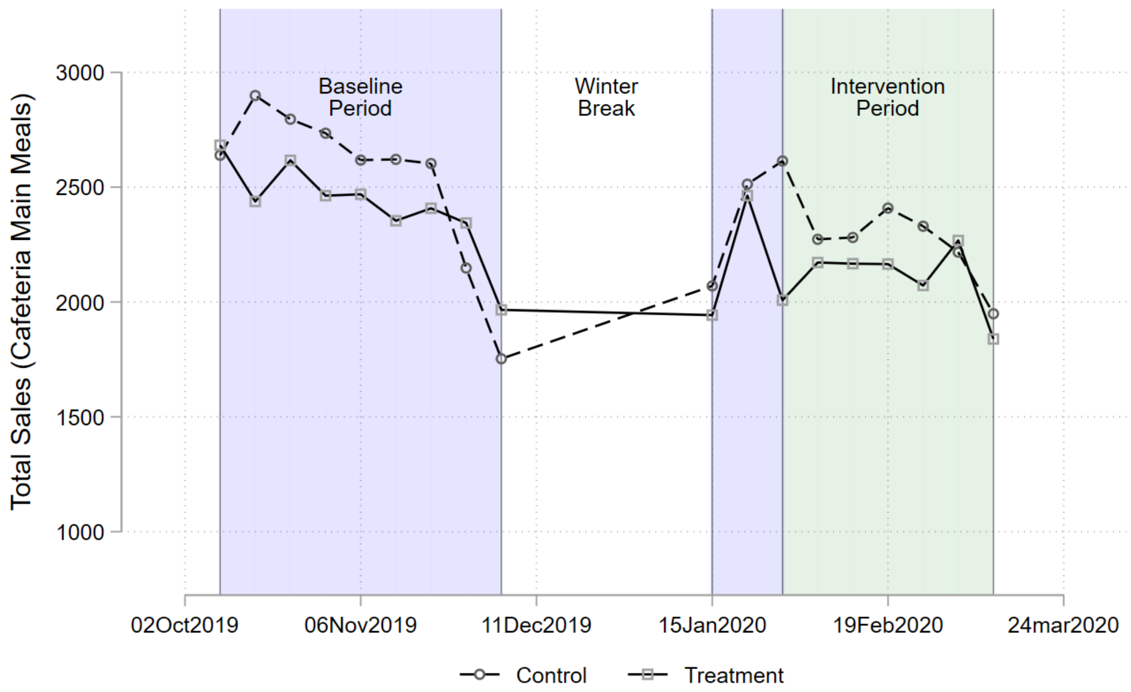

Figure 4.2.3: Total weekly cafeteria main meals sold in treatment and control cafeterias

Note: Based on $N = 84,307$ individual sales.

To provide further insights into potential mechanisms, we collected qualitative data via an exit survey conducted among customers of all three treatment cafeterias at the end of the intervention period. The exit survey was designed to collect basic demographic information and assess how labels were perceived with respect to their credibility, importance, comprehensiveness and usefulness. Participants were also asked to self-report in which situations the labels influenced their choices. Finally, participants had the option to provide consent for their responses to be linked to sales data using anonymised identifiers. The surveys were distributed via each college's mailing lists and participation was incentivised with a prize draw.

4.2.5 Outcome variables and hypotheses

Our data allows us to explore the impact of treatment (exposure to carbon label) on multiple outcome variables related to whether participants make more climatarian meal choices. To do so, we construct three binary outcome variables equal to one if an individual selected a low-carbon impact meal with less than 250g CO_2 per 100g serving, mid-carbon impact meal between 250g and 500g CO_2 per 100g serving or a high-carbon impact meal with more than > 500g CO_2 per 100g.[14] Low-carbon impact meals thus correspond to dark-green and light-green labelled meals, mid-carbon meals represent yellow-labelled meals and high-carbon meals combine orange and red-labelled meals. The label categories were aggregated for the main analysis to facilitate the presentation and interpretation of our findings. In a supplementary analysis (Section 4.3.5), we explore effects of the labels for each of the five label colours. Moreover, to directly estimate the effect of the labelling intervention on the average carbon footprint of meal choices, we construct a continuous outcome variable for the CO_2 content of each meal choice.

Previous literature on interventions to reduce meat consumption has primarily focused on how the share of vegetarian/vegan dishes responds to an intervention (Garnett et al., 2019; Garnett et al., 2020; Kurz, 2018). To provide comparable results, we construct an aggregated binary indicator equal to one if an individual selected a meat or fish dish and zero if the vegan or vegetarian alternative was chosen. However, it is important to note that using meat/fish or vegetarian/vegan choices as a proxy for more sustainable food choices may mask some of the complexities associated with the carbon footprint of different foods. In particular, vegetarian dishes in our data are found across all five label-colour categories. Similarly, fish dishes have an equally diverse range of carbon footprints (excluding the lowest-carbon category). Hence, utilising aggregate choice variables may not accurately capture changes in preferences for climatarian diets (i.e., diets specifically aimed at reducing the carbon footprint). We thus provide a more nuanced analysis in Section 4.3.5 in which the choice setting is defined as a choice between vegan, vegetarian, fish and meat alternatives. Moreover, we allow the treatment effect to vary corresponding to the label colour with which each alternative is labelled.

We expect carbon footprint labels to decrease the market share of high-impact carbon meals and shift consumer preferences to more sustainable options. Both hypotheses are based on findings from previous research suggesting that carbon labels on food items aid customers to make more sustainable consumption choices (Brunner et al., 2018; Camilleri et al., 2019). We expect this effect to be partially reflected by a decrease in meat/fish sales and an increased demand for vegan/vegetarian meal options.

[14] In each case, the comparison group encompasses all other available alternatives.

4.2.6 Estimation strategy

To estimate the effect of carbon footprint labels on the previously discussed outcome variables we implement an incremental estimation strategy, with each estimation step building the case for the robustness of our analysis. First, our main results come from a generalised difference-in-differences (DID) model with unit and time fixed effects to control for individual-specific heterogeneity and any exogenous factors that could affect food choices during the experimental period, as described in Baker et al. (2022).

$$Y_{its} = \alpha_i + \lambda_t + \delta^{DD} D_{is} + X_s + \varepsilon_{ist} \tag{4.1}$$

where Y_{it} is the binary dependent variable of meal choice or continuous variable of the carbon footprint of a particular meal choice made by individual i at cafeteria service s in week t. Individual fixed effects are captured by α_i and λ_t are week fixed effects. Individual fixed effects account for unobserved preference heterogeneity such as dietary preferences or restrictions and week fixed effects capture common shocks over time (e.g., midterm exams). X_s is a vector of control variables specific to cafeteria service s. Controls include day-of-week dummies, an indicator for dinner services, the total number of sales and total number of options available at cafeteria service s, as well as the hourly temperature. For binary meal choice indicators, we also control for the number of options available for the dependent variable in question and the average price differential between high and low-carbon alternatives. Both availability and price have been found to play an important role in food purchase decisions (Garnett et al., 2021; Garnett et al., 2019).[15]

$D_{is} = Treat \times Post$ is an indicator for a treated individual (Treat) during the labelling period (Post), with both main effects being subsumed by the individual and week fixed effects. The coefficient of interest is the DID estimator δ^{DD} which is unbiased in settings where there is a single treatment (Baker et al., 2022). We estimate linear probability models of equation (4.1) by OLS for each binary meal choice outcome (Y_{it}) separately and exclude observations where Y_{it} was either not available to choose as an alternative, or the only option available. The effective sample size thus varies depending on which dependent variable is analysed.

To probe the robustness of equation (4.1), we additionally model individual purchase decisions within a random utility maximisation framework using random-parameter mixed logit (MXL) models. The panel-data MXL models the probability of selecting each alternative for each

[15] Note that the price-differential between meat/fish and vegan/vegetarian options is used in the analysis of meat/fish choices.

choice situation (cafeteria service). Importantly, the MXL model uses random coefficients to relax the Independence-of-Irrelevant Alternatives (IIA) assumption, a restrictive assumption which implies that the error terms cannot be correlated across alternatives or over time. Moreover, the MXL model has the ability to account for preference heterogeneity by allowing the utility parameters to flexibly vary across choice makers (Train, 2009). For instance, individuals may hold heterogeneous preferences over the carbon-impact of meals, the meal-type itself and may be subject to varying degrees of price sensitivity. Formally, we model the probability (P^j_{it}) that alternative j is selected by individual i at cafeteria service s.

$$P^j_{is} = Prob\left[Y_{is} = j\right] = \frac{exp\left(\beta_{0,j}ASC^j + \beta_1 price^j_{is} + \beta_2 nroptions^j_{is} + \rho Treat + \gamma Post + \delta^{DD}\left(Treat \times Post\right) + X_{is} + \varepsilon_{is}\right)}{\Sigma^j_{k=1} exp\left(\beta_{0,j}ASC^j + \beta_1 price^J_{is} + \beta_2 nroptions^J_{is} + \rho Treat + \gamma Post + \delta^{DD}\left(Treat \times Post\right) + X_{is} + \varepsilon_{is}\right)} \quad (4.2)$$

Corresponding to the binary outcome variables discussed above we consider two specifications of equation (4.2), the first with three alternatives (j) capturing low, mid and high-carbon dishes and the second with two alternatives for meat/fish and vegan/veggie dishes. We include alternative-specific constants (ASC^j) and two alternative-specific variables for the price of each option ($price^j_{is}$) and the availability of each meal option ($nroptions^j_{is}$). In addition, we include a set of case-specific controls as in equation (4.1).[16] To identify the DID estimator δ^{DD} capturing the treatment effect of the labelling intervention, we use a standard DID specification with indicator variables $Post$ for observations in the intervention period and Treat for sales recorded in treatment cafeterias.[17] To account for unobserved preference heterogeneity, we allow the alternative-specific constants (ASC) as well as the price attribute to be randomly distributed in the population (Hensher & Greene, 2003; Train, 2009). All MXL models are estimated via simulated maximum likelihood with 300 Halton draws. To obtain interpretable estimates, we compute the marginal treatment effects following Puhani (2012) and account for unbalanced choice sets by restricting the sample for each alternative to the subpopulation of cases which include that alternative in their choice set.[18]

[16]Note that in equation (4.2) the vector of control variables X_{it} no longer includes availability and price differential controls as these are now directly captured by the alternative-specific variables $price$ and $nroptions$.

[17]We estimate a standard DID model to avoid computational difficulties when estimating conditional and mixed-logit models, resulting from the inclusion of a large number of unit and time fixed effects.

[18]Yet, despite the advantages of the MXL model to analyse choice data, estimation via maximum-likelihood simulation is computationally demanding if the number of choice sets or covariates is large. We are, therefore, required to exclude individual and week fixed effects from our MXL specifications, although these may capture important unobserved individual characteristics as well as common temporal shocks, thereby making our estimates more precise. For this reason, we will discuss equation (4.1) estimated by OLS as providing the main results and equation (4.2) as providing the basis for our robustness analysis.

To explore the more nuanced effects of carbon footprint labels on meal choices, we consider a choice setting in which consumers choose between four alternatives: vegan, vegetarian, fish and meat. Moreover, we extend equation (4.2) to allow the treatment effect on the choice probability of alternative j to vary by label colour. The mixed logit model takes the following form:

$$P_{is}^{j} = Prob\left[Y_{is} = j\right] = \frac{exp(\beta_{0,j} ASC^{j} + \beta_1 price_{is}^{j} + \beta_2 nroptions_{is}^{j} + \beta_3 label^{j} + \beta_4(label^{j} \times T) + \beta_5(label^{j} \times P) + \beta_6(label^{j} \times T \times P) + \rho T + \gamma P + \delta^{DD}(T \times P) + X_{is} + \eta_{is}}{\Sigma_{k=1}^{j} exp(\beta_{0,j} ASC^{j} + \beta_1 price_{is}^{j} + \beta_2 nroptions_{is}^{j} + \beta_3 label^{j} + \beta_4(label^{j} \times T) + \beta_5(label^{j} \times P) + \beta_6(label^{j} \times T \times P) + \rho T + \gamma P + \delta^{DD}(T \times P) + X_{is} + \eta_{is})} \tag{4.3}$$

where $label^{J}$ represents a vector of four indicator variables for each label colour of alternative j in choice situation t (yellow is omitted as the base-category) and T and P are abbreviations for $Treat$ and $Post$, respectively. We estimate the marginal effects of the intervention for each combination of alternative and label-colour by restricting the sample to subpopulations of cases in which the respective combination was part of the choice set.

4.2.7 Statistical inference

Following Bertrand et al. (2004), we cluster standard errors at the individual level to account for within-individual error correlations. In the context of food choices, accounting for within-individual serial correlation is important due to differences in diet preferences and tastes, as well as cultural dietary restrictions or allergies. However, we may also be concerned about clustering at the cafeteria level as each cafeteria has slightly different practices, menus and employs different chefs. Moreover, individual diners are assigned to treatment at the cafeteria-level which justifies cluster-adjustments at this level (Abadie et al., 2017). In section 4.3.3 we explore the robustness of our main results to clustering at the cafeteria-level by implementing the wild bootstrap-t procedure to account for the small number of clusters (Cameron et al., 2008; Roodman et al., 2019).

Identification in DID analysis depends crucially on the assumption that both treatment and control group would follow the same trend in outcomes, in the absence of an intervention. Whilst this assumption is not directly testable, we are able to draw on our 9-week baseline period to explore whether meal choices followed similar trends in treatment and control groups prior to the intervention. Appendix Figures 4.B1-4.B5 plot the raw data for treatment and control groups showing the average weekly sales of each dependent variable. The samples used to plot average weekly sales for a given dependent variable are restricted to the respective observations employed in the main analysis. A visual assessment of the pre-trends suggests that both treatment and control groups follow a comparable pre-intervention trend for all key outcome variables. However, it is also apparent that average sales are highly volatile on a week-by-week basis. This variation is likely due to unobserved differences in popularity of certain dishes available in a given week. As treatment and control cafeterias do not follow the same menus, it is to be expected that trends differ between treatment and control group on a weekly basis. However, over the entire baseline period, trends appear to follow a common trajectory. To provide greater clarity whether long-run trends are comparable between treatment and control groups, we perform a formal statistical test for the equality of pre-trends using data from the pre-intervention period between October 2019 and January 2020. We estimate a model with the previously discussed indicators for meal choices as the dependent variables, regressed on the same set of controls and fixed effects specified in equation (4.1), as well as a linear time trend (number of cafeteria services since 7$^{\text{th}}$ October) and its interaction with the treatment group indicator. The parameter of interest is the estimated coefficient on the interaction term between the linear time-trend and the treatment group dummy. The results from this exercise are shown in Appendix Table 4.B1. We find no statistically detectable difference in the trends prior to the labelling intervention for treatment and control cafeterias.

4.3 Results

4.3.1 Descriptive statistics

Table 4.3.1 provides summary statistics of the full sample, including all observations made during the study period. We observe a total of 40,839 and 43,468 individual purchase decisions in the treatment and control groups, respectively. The full sample spans observations from 84,307 individual meal choices, made by 2,682 individuals during 232 cafeteria services (i.e., lunch or dinner services) over a period of 125 days. Students visited the cafeteria on average 31 times over the entire study period, equivalent to consuming either lunch or dinner in the cafeteria twice per week. Table 4.3.1 shows that observations are evenly distributed across treatment and control cafeterias.

Table 4.3.1: Treatment and control sample statistics over the experimental period

	Treatment cafeterias		Control Cafeterias	
	Baseline	Intervention	Baseline	Intervention
Days	77	48	76	49
Cafeteria Services	143	90	139	90
Individuals	1,379	1,151	1,241	1,139
Mean Visits per Individual	19	13	22	14
Individual Sales	26,148	14,691	27,395	16,073
Total Sales	N=40,839		N=43,468	

Note: Table provides an overview of the analysis sample using all available meal choice observations ($N = 84,307$). Cafeteria services refers to the number of mealtimes (i.e. lunch and dinner services).

Table 4.3.2 shows the meal sales shares for our main outcome variables for both the baseline and intervention period across treatment and control cafeterias. The sample used to compute the sales share for a given dependent variable is restricted to those observations where the respective meal option was available as one of multiple options and the exact choice could be observed. If a high-carbon meal was available, it was chosen approximately 50% of the time in treatment cafeterias and slightly more frequently in the control cafeterias (57%) during the baseline period. Moreover, approximately every third meal choice in the treatment cafeterias was a low-carbon or mid-carbon meal if these options were available to choose from. In the control cafeterias, low- and mid-carbon meals were slightly less popular, making up 29% and 25% of choices in the baseline period, respectively. Meat and fish meals were consistently more popular than vegan and vegetarian alternatives. Between baseline and intervention periods, we observe minor changes in the sales shares of treated cafeterias in the expected directions (decrease in high-carbon sales, increases in mid and low-carbon sales) while in the control cafeterias we observe changes in the opposite direction.

Table 4.3.2: Share of dishes sold in treatment and control cafeterias over the experimental period

	Treatment cafeterias				Control Cafeterias			
Meal Sales Share	Baseline		Intervention		Baseline		Intervention	
Low-Carbon	0.33	(0.47)	0.34	(0.47)	0.29	(0.45)	0.26	(0.44)
Mid-Carbon	0.31	(0.46)	0.33	(0.47)	0.25	(0.43)	0.24	(0.43)
High-Carbon	0.52	(0.50)	0.50	(0.50)	0.57	(0.49)	0.59	(0.49)
Meat/Fish	0.59	(0.49)	0.59	(0.49)	0.63	(0.48)	0.65	(0.48)
Vegan/Vegetarian	0.41	(0.49)	0.41	(0.49)	0.37	(0.48)	0.35	(0.48)
Carbon Footprint	571.01	(517.48)	564.57	(525.51)	670.73	(635.18)	687.47	(631.53)

Note: Table shows the percentage of meals *sold* for each dependent variable during the entire baseline and intervention period in both treatment and control cafeterias. "Low-Carbon" includes meals labelled dark and light green; "Mid-Carbon" includes yellow labelled meals and "High-Carbon" includes orange and red labelled meals (see Figure 4.2.1). Carbon Footprint in grams of CO_2 equivalent per 100g serving. Meal shares are computed based on observations where the exact meal choice could be identified as one of multiple alternatives for each respective dependent variable (i.e., excluding observations where the dependent variable was not available). For this reason, the sum of meal sales shares for low, mid and high-carbon alternatives is greater than 1. Standard deviation in parentheses.

4.3.2 Main results

We first examine the average treatment effect of the labelling intervention obtained by estimating equation (4.1) and substituting the outcome variables described in Section 4.2.5 to explore changes in both climatarian and vegetarian meal choices. Figure 4.3.1 visualises the average treatment effects, while Table 4.3.3 presents results for the main coefficients of interest (the full results are shown in Appendix 4.C). Columns (1) to (3) in Table 4.3.3 show the effects of carbon footprint labels on the probability of selecting a low, mid or high-carbon impact meal. Note that the three categories correspond to the label colours representing a range of CO_2 emissions: 'Low' combines choices of dark green and light green meals (< 250g CO_2 per 100g), 'mid' represents yellow-labelled (250g-500g CO_2 per 100g) meals and high encompasses orange and red labelled meals (> 500g CO_2 per 100g). Column (4) shows the average treatment effect of the labels on the carbon footprint of meal choices. Columns (1) to (4) reflect climatarian preferences and are estimated using data from the four cafeterias in which we observe individuals' exact meal choices. Finally, column (5) shows the effects of carbon footprint labels on the likelihood of selecting fish or meat dish, estimated utilising data from all five cafeterias.

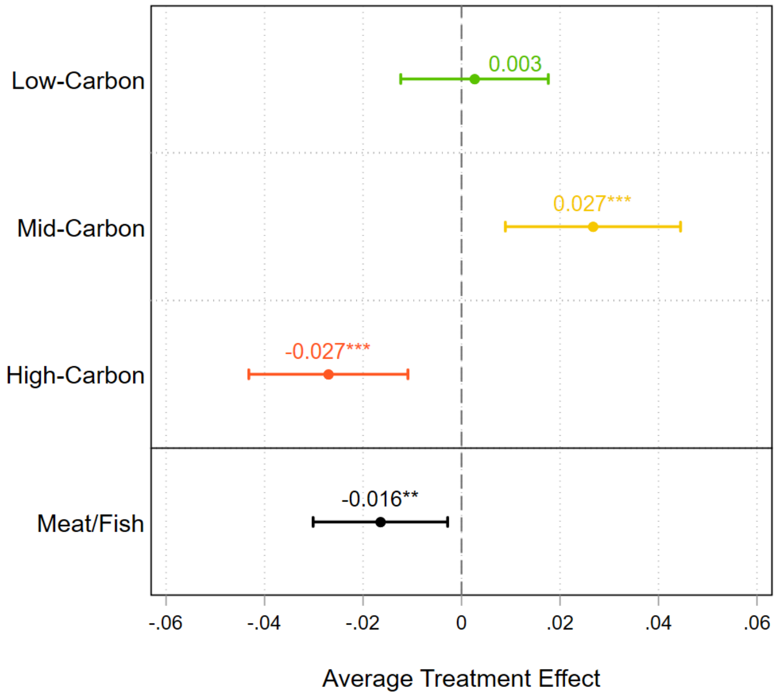

Figure 4.3.1: Average treatment effects

Note: OLS estimates of equation (4.1). The dependent variables are binary meal choice indicators for low-, mid-, high-carbon and meat/fish choice. Error bars indicate 95% confidence intervals. Full model results shown in Appendix Table 4.C1.

Focusing on our main dependent variables capturing climatarian preferences (columns 1 – 4) we find evidence that carbon footprint labels decreased the market share of high-carbon impact meals by 2.7 percentage points and led to a corresponding increase (2.7 percentage points) in the share of mid-carbon impact meals. Both estimates are highly statistically significant at the 1% level. We find no effect of the labels on low-carbon meal choices. Column (4) reports the effect of the labelling intervention on a continuous variable capturing the carbon footprint of meal choices. The negative coefficient indicates that carbon footprint labels caused a reduction of 27g CO_2 in the average footprint consumed per 100g serving, significant at the 1% level.

With respect to preferences for the meat/fish alternative (column 5), we find that carbon footprint labels caused a decrease in the market share of fish and meat sales by 1.6 percentage points which corresponds to an increase in vegan and vegetarian sales by the same amount, significant at the 5% level. However, it is important to note that an aggregate indicator combining both meat and fish masks important substitution patterns between meat and fish

Table 4.3.3: Main results

	Climatarian Preferences				
	(1) Low	(2) Mid	(3) High	(4) GHG	(5) Fish/Meat
Post × Treat	0.003	0.027***	-0.027***	-26.786***	-0.016**
	(0.008)	(0.009)	(0.008)	(8.800)	(0.007)
ID & Week FE	Yes	Yes	Yes	Yes	Yes
Individuals	2,005	1,899	2,014	2,043	2,672
Observations	58,006	39,672	58,612	61,239	78,393

Note: OLS estimates of equation (4.1). The dependent variables in columns (1)-(3) are indicators for low, mid and high-carbon meal choice, respectively, and zero if any other alternative was chosen. The dependent variable in column (4) is a continuous variable for the carbon footprint of meal choice. The dependent variable in column (5) is an indicator for fish/meat meal choice. Post × Treat is the difference-in-differences estimator (δ^{DD}) capturing the treatment effect. Controls include total sales, total number of options available, number of options available of Y (for binary meal choice indicators), price differential between veg and meat or high-carbon and low-carbon alternatives, indicator for dinner service, hourly temperature and day-of-week dummies. All models include individual and week fixed effects. Standard errors clustered at the individual level in parentheses. Full model results shown in Appendix 4.C.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

alternatives. More nuanced results of the treatment effect on both meat and fish alternatives are provided in Section 4.3.5.

4.3.3 Robustness

Mixed-logit choice model

This section demonstrates robustness of our main results when estimated via simulated maximum likelihood using a random parameter mixed-logit model as specified in equation (4.2). Table 4.3.4 presents the marginal effects of the labelling intervention on the choice probabilities for each of the outcomes which are selected from a set of possible alternatives. Columns (1) to (3) are obtained from a MXL model, in which the choice alternatives are defined as low, mid and high-carbon impact alternatives. Column (4) reflects a binary choice setting in which the consumer chooses between vegan/vegetarian or fish/meat alternatives.

Table 4.3.4: Robustness: Mixed-logit estimates

	(1) Low	(2) Mid	(3) High	(4) Fish/Meat
Post × Treat	0.010	0.016*	-0.021***	-0.017***
	(0.007)	(0.009)	(0.007)	(0.006)
ID & Week FE				
Observations	157,184	157,184	157,184	157,470
Nr. Cases	61,395	61,395	61,395	78,735

Note: Mixed-logit estimates of equation (4.2). Marginal effects are computed for each alternative based on the subpopulation of cases with that alternative in their choice set. The choice alternatives in columns (1) to (3) are defined as low, mid and high carbon meals. The base category is the mid-carbon meal alternative which is constrained to zero. Column (4) is based on the binary choice scenario between Fish/Meat and Vegan/Vegetarian alternatives and the base category is meat/fish. Alternative specific attributes account for the price and availability of each alternative. Additional case-specific covariates include controls for total sales, total number of options available, average hourly temperature, an indicator for dinner service and day-of-week dummies. Standard errors clustered at the individual level in parentheses. Full model results shown in Appendix Tables 4.C2 and 4.C3.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We find comparable, yet slightly smaller marginal treatment effects (Post x Treat) across all four dependent variables. On average, carbon footprint labels increased the probability of selecting a mid-carbon meal by 1.6 percentage points (Column 2) and decreased the probability of selecting a high-carbon meal by 2.1 percentage points (Column 3), significant at the 10% and 1% level, respectively. The findings thus confirm the substitution pattern from high to mid-carbon alternatives shown in Section 4.3.2, which is reflected by a reduction in the probability of selecting a fish or meat dish (Column 4). Appendix 4.C reports the full model results for the mixed-logit specifications in Tables 4.C2 and 4.C3. Both appendix tables additionally present the estimated standard deviations for the alternative specific constants and price variables obtained from the mixed-logit model. The estimated means and standard errors indicate that there is a highly statistically significant degree of preference heterogeneity in our sample, suggesting that individuals vary in their level of appreciation of low and high carbon meals, as well as vegetarian dishes. We also document a significant negative correlation between low and high-carbon alternatives, significant at the 1% level.

Our analysis, so far, has focused on examining the average treatment effects of the intervention on food choices, which provide a useful indication of the overall efficacy of carbon footprint labelling. However, an interesting question relates to whether effects differ with changes in the choice set composition. For instance, one might explore whether consumers substitute from high-carbon to low-carbon dishes if no mid-carbon impact dish is available to choose from. The mixed-logit choice model allows us to conveniently estimate the marginal treatment effects for each possible combination of low, mid and high-carbon alternatives (i.e., the different choice sets which occur in our data). This is achieved by restricting the sample to those cases that constitute a specific choice set and estimating the corresponding marginal effects with this subpopulation of observations. In our data, all three alternatives were available in 34,394 choice situations ($N = 103,182$). In 2,494 cases ($N = 4,988$) only low and mid-carbon alternatives were available. In 21,408 cases ($N = 42,816$) only low and high alternatives were available and in the remaining 3,099 cases ($N = 6,198$) only mid and high-carbon dishes were available to choose from. The results from this analysis are presented in Table 4.3.5.

Table 4.3.5: Robustness: Climatarian preferences by choice set

	(1) Low	(2) Mid	(3) High
A: Low & Mid & High (N = 103,182)			
Post × Treat	0.007	0.016*	-0.023***
	(0.008)	(0.009)	(0.008)
B: Low & Mid (N = 4,988)			
Post × Treat	-0.005	0.005	
	(0.010)	(0.010)	
C: Low & High (N = 42,816)			
Post × Treat	0.017**		-0.017**
	(0.007)		(0.007)
D: Mid & High (N = 6,198)			
Post × Treat		0.026***	-0.026***
		(0.010)	(0.010)

Note: Mixed-logit estimates of equation (4.2). The choice alternatives in columns (1) to (3) are defined as low, mid and high carbon meals, respectively. The base category is the mid-carbon meal alternative which is constrained to zero. Panels A, B, C and D display the marginal effects estimated for sub-populations grouped by available choice set. The sample in Panel A is restricted to choice sets in which all three alternatives are available. The samples in Panels B, C and D are restricted to choice sets in which one of the three alternatives is not available, thus resulting in different combinations of low, mid and high alternatives. Post × Treat is the marginal effect capturing the treatment effect. Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Several interesting findings emerge. First, we find that labels decreased the choice probability of selecting a high-carbon meal regardless of which other alternatives were available to choose from. Interestingly, the substitution effect is most pronounced (2.6 percentage points) if the only other alternatives were mid-carbon meals (Panel D). If all three alternatives were available (Panel A) the decrease in the choice probability for high-carbon meals was 2.3 percentage points. However, if no mid-carbon meals were available in the choice set, consumers still decreased their consumption of high-carbon meals by 1.7 percentage points and increased low-carbon meal choices by 1.7 percentage points. These findings suggest that, if at least one mid-carbon option is available, consumers will prefer to switch from high-carbon to the mid-carbon alternatives. However, if only high-carbon and low-carbon alternatives are available on the menu, labels retain their effect but to a lesser extent. If only low and mid-carbon alternatives were available (Panel B), labels had no impact on consumer choice.

Additional robustness checks

Appendix 4.A contains additional robustness checks and supplementary analysis. Table 4.A2 shows the main results obtained from an adjusted version of equation (4.1) which excludes all time-varying controls (X_s). With this specification, we find that some of the treatment effects are substantially larger than those presented in Section 4.3.2 and all estimates are highly statistically significant. Moreover, we observe a statistically significant positive effect of labels on choices of low-carbon dishes, which is not encountered in our main results. These additional findings point to the importance of controlling for time-varying factors at the cafeteria-service level. Moreover, we note that our main results are unchanged if the sample is restricted to weekdays only.

Table 4.A3 shows the p-values for the DID estimator obtained from estimating (4.1) using both our preferred clustering approach (on the individual diner) and wild bootstrap clustered standard errors at the cafeteria level. We recover comparable p-values and the statistical significance of our main results presented in Section 4.3.2 remain statistically significant at the 5% level for vegetarian choice variables and at the 10% level for climatarian choice variables. However, we find that the estimate for the continuous carbon footprint outcome variable does not reach statistical significance when clustering at the cafeteria-level.

Rebound effects and attrition

A further concern relates to the possibility that the introduction of carbon footprint labels leads to avoidance behaviour and may deter individuals from dining at the cafeteria. Not only may this result in potential behavioural rebound effects (e.g. individuals consuming a high-carbon meal elsewhere), but may also lead to selective attrition over time if certain groups of individuals are differentially affected by the labels. For instance, if labels lead to avoidance behaviour amongst individuals who follow a high-carbon impact diet prior to the intervention (e.g. by evoking feelings of guilt), our estimates would no longer be unbiased. We address the first concern by aggregating the sales data to the "cafeteria service" level for each cafeteria (see Table 4.3.1) and estimating an adapted version of equation (4.1) with total meal sales at a given cafeteria service as the dependent variable.[19] Results are shown in column (1) of Appendix Table 4.A4 and suggest that there is no statistically significant effect of the label intervention on total sales.

[19] As the data are aggregated at the cafeteria service level, subscript i as well as the individual fixed effects are removed. We control for additional external factors that may influence total sales, including average rainfall and humidity during the cafeteria service.

To address the second concern, we assign each individual to one of four equally sized groups, ranging from 'Least Green' to 'Most Green', based on their frequency of low-carbon meal consumption during the baseline period (discussed in detail in the following section). We then aggregate the data at the 'cafeteria service' level and calculate the share of customers in each quartile relative to the total customers for each cafeteria service. Next, we estimate the DID model with each share as the dependent variable. The DID estimator indicates whether the labelling intervention caused an increase or decrease in the share of customers of each category. Results are shown in columns (2) to (5) of Appendix Table 4.A4. We find that the introduction of carbon footprint labels had no effect on the share of customers in each preference quartile, and conclude that differential attrition does not threaten the internal validity of our results.

4.3.4 Preference heterogeneity

In this section we explore whether carbon footprint labels have heterogeneous effects on meal choices for individuals with different pre-intervention dietary habits. We classify frequent customers (with at least 10 cafeteria visits during the baseline period) into eight groups based on their pre-intervention consumption patterns.[20] Specifically, we assigned individuals into four equally sized groups ranging from 'Least Veg' to 'Most Veg', based on how frequently they consumed vegetarian or vegan meals during the baseline period. Additionally, we defined four climatarian preference quartiles based on the frequency of low-carbon meal consumption ranging from "Least Green", those individuals who were least likely to choose a low-carbon alternative (<250g CO_2 per 100g serving), to "Most Green" which captures those individuals who already pre-dominantly followed a low-carbon footprint diet. Tables 4.3.6 and 4.3.7 show the estimated average treatment effect of the carbon footprint labels on climatarian meal choices for each sub-sample of vegetarian and climatarian preferences, respectively, estimated by OLS using the generalised DID specification shown in (4.1).

[20]Following Garnett et al. (2019), we selected 10 observations as the minimum number of cafeteria visits to accurately classify individual dietary preferences. While setting a higher threshold would further increase accuracy, this would result in a smaller sample size by restricting the sample to the more frequent customers. Ten cafeteria visits correspond to approximately one visit per week over the entire baseline period and thus should offer a varied picture of people's food preferences.

Table 4.3.6: Heterogeneity analysis: Vegetarian baseline preferences

	(1) Least Veg	(2) Less Veg	(3) More Veg	(4) Most Veg
Panel A: DV = Low Carbon Choices				
Post × Treat	-0.007	0.023*	0.022	0.023
	(0.012)	(0.013)	(0.021)	(0.017)
Obs.	14,859	13,645	12,476	12,338
Panel B: DV = Mid Carbon Choices				
Post × Treat	0.067***	0.029	0.014	-0.033*
	(0.017)	(0.018)	(0.021)	(0.018)
Obs.	10,516	9,376	8,312	8,151
Panel C: DV = High Carbon Choices				
Post × Treat	-0.042***	-0.041**	-0.038*	-0.006
	(0.016)	(0.017)	(0.020)	(0.015)
Obs.	15,284	13,809	12,487	12,249
ID & Week FE	Yes	Yes	Yes	Yes

Note: Results obtained from 12 separate OLS regressions of equation (4.1) using four equally sized subsamples (quartiles) grouping individuals by pre-intervention consumption frequency of vegan/vegetarian meals. The dependent variable (DV) in Panel A, B and C capture low, mid and high-carbon meal choices, respectively. Post × Treat is the difference-in-differences estimator (δ^{DD}) capturing the treatment effect. Controls include total sales, total number of options available, number of options available for Y, price differential between veg and meat or high-carbon and low-carbon alternatives, indicator for dinner service, hourly temperature and day-of-week dummies. All models include individual and week fixed effects. Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

With respect to baseline vegetarian preferences, the results indicate that the carbon footprint labels had the largest effect on individuals who were least likely to consume vegetarian or vegan meals in the baseline period (i.e., followed a meat-heavy diet). Individuals in this preference quartile (Least Veg) decreased their consumption of high-carbon meals by on average 4.2 percentage points and increased mid-carbon meal consumption by 6.7 percentage points, with both estimates statistically significant at the 1% level. Individuals in the second and third preference quartiles (Less Veg & More Veg) showed a comparable decrease in high-carbon meals (4.1 & 3.8 percentage points), significant at the 5% and 10% level, respectively. Interestingly, those individuals who already followed a predominantly vegetarian diet (Most Veg) significantly decreased their consumption of mid-carbon impact meals, significant at the 10% level.

Table 4.3.7: Heterogeneity analysis: Climatarian baseline preferences

	(1) Least Green	(2) Less Green	(3) More Green	(4) Most Green
Panel A: DV = Low Carbon Choices				
Post × Treat	-0.001	0.022*	0.016	0.028
	(0.012)	(0.013)	(0.018)	(0.018)
Obs.	13,123	13,826	13,028	12,749
Panel B: DV = Mid Carbon Choices				
Post × Treat	0.044**	0.041**	0.009	-0.018
	(0.017)	(0.018)	(0.020)	(0.018)
Obs.	9,231	9,379	8,942	8,420
Panel C: DV = High Carbon Choices				
Post × Treat	-0.033*	-0.049***	-0.032*	-0.021
	(0.017)	(0.016)	(0.019)	(0.015)
Obs.	13,475	13,968	13,131	12,656
ID & Week FE	Yes	Yes	Yes	Yes

Note: Standard errors clustered at the individual level in parentheses. Results obtained from 12 separate OLS regressions of equation (4.1) using four equally sized subsamples (quartiles) grouping individuals by pre-intervention consumption frequency of low-carbon (green) impact meals. The dependent variable (DV) in Panel A, B and C capture low, mid and high-carbon meal choices, respectively. Post × Treat is the difference-in-differences estimator (δ^{DD}) capturing the treatment effect. Controls include total sales, total number of options available, number of options available for Y, price differential between veg and meat or high-carbon and low-carbon alternatives, indicator for dinner service, hourly temperature and day-of-week dummies. All models include individual and week fixed effects. Standard errors clustered at the individual level in parentheses.

* p < 0.1, ** p < 0.05, *** p < 0.01.

As for climatarian preferences, the results indicate that individuals who followed a predominantly high-carbon footprint diet (i.e. consumed low-carbon meals less or least frequently in the pre-intervention period) were most likely to change their behaviour due to the carbon footprint labels. We observe a 3.3 percentage point decrease in the probability of selecting a high carbon meal for the “Least Green” preference quartile and a 4.9 percentage point decrease for the “Less Green” quartile, with estimates being statistically significant at the 10% and 1% significance level, respectively. Both groups increased their consumption of mid-carbon meals (4.4 and 4.1 percentage points, significant at the 5% level), whereas the “Less Green” preference quartile also increased consumption of low-carbon meals by 2.2 percentage points, significant at the 10% level.

4.3.5 Treatment effect by label colour

The results presented so far show that carbon footprint labels encourage consumers to substitute high-carbon meals with mid-carbon alternatives, which is reflected by a decrease in meat/fish sales and an increase in vegan/vegetarian choices. However, as previously discussed, aggregating vegan and vegetarian, and fish and meat choices into two categories may mask some of the nuances related to food preferences. Furthermore, we may suspect the treatment effect to differ depending on the colour with which dishes of the same category are labelled (i.e., their relative carbon footprint). For instance, the effect of the label may differ between yellow and red labelled fish dishes. To shed light on the more nuanced effects of carbon footprint labels on meal choices, we estimate (using equation (4.3)) the marginal effects of the treatment on the predicted probabilities of vegan, vegetarian, fish and meat choices for different sub-populations of choice scenarios representing all possible combinations of each alternative and label colour. Results are shown in Figure 4.3.2.

In line with our expectations, the results indicate that the treatment effect depends on the relative carbon footprint of the meal and the corresponding label colour with which it was labelled. We find that labels increased the sales of vegan meals if they were labelled light-green or yellow by approximately 3.1 percentage points (significant at the 5% and 10% level, respectively) but had no effect on vegan meals in the dark-green category. This finding suggests that the relatively higher carbon vegan alternatives gained in popularity, but choices of the lowest carbon vegan meals were unaffected by the labels and even display a slight decrease. Turning to vegetarian sales, we observe that these remain largely unchanged by carbon footprint labels, apart from sales of yellow labelled vegetarian meals which increased by 2 percentage points, significant at the 10% level. With respect to fish dishes, labels caused an increase of 2.2 percentage points in the sales of light-green labelled fish meals and an even larger increase (2.6 percentage points) if they were labelled with yellow, significant at the 10% and 1% level, respectively. However, the labels had no statistically significant effect on choices of orange and red-labelled fish dishes. Finally, labels led to a 3-percentage point decrease in orange labelled meat dishes, significant at the 1% level, and a 1.8-percentage point decrease in red-labelled meat dishes, significant at the 10% level. Choices of meat dishes in the mid-carbon range were unaffected by the labels. Taken together, these results confirm our hypothesis that the efficacy of labels in changing meal choices is specific to the type of meal and with which colour it is labelled. The results provide an additional layer of detail to aid our interpretation of the main findings presented in Section 4.3.2. We observe that the increase in mid-carbon impact (yellow-labelled) meals stems from increased sales of vegan, vegetarian and fish dishes, whereas the reduction in high-carbon sales is primarily driven by a decrease

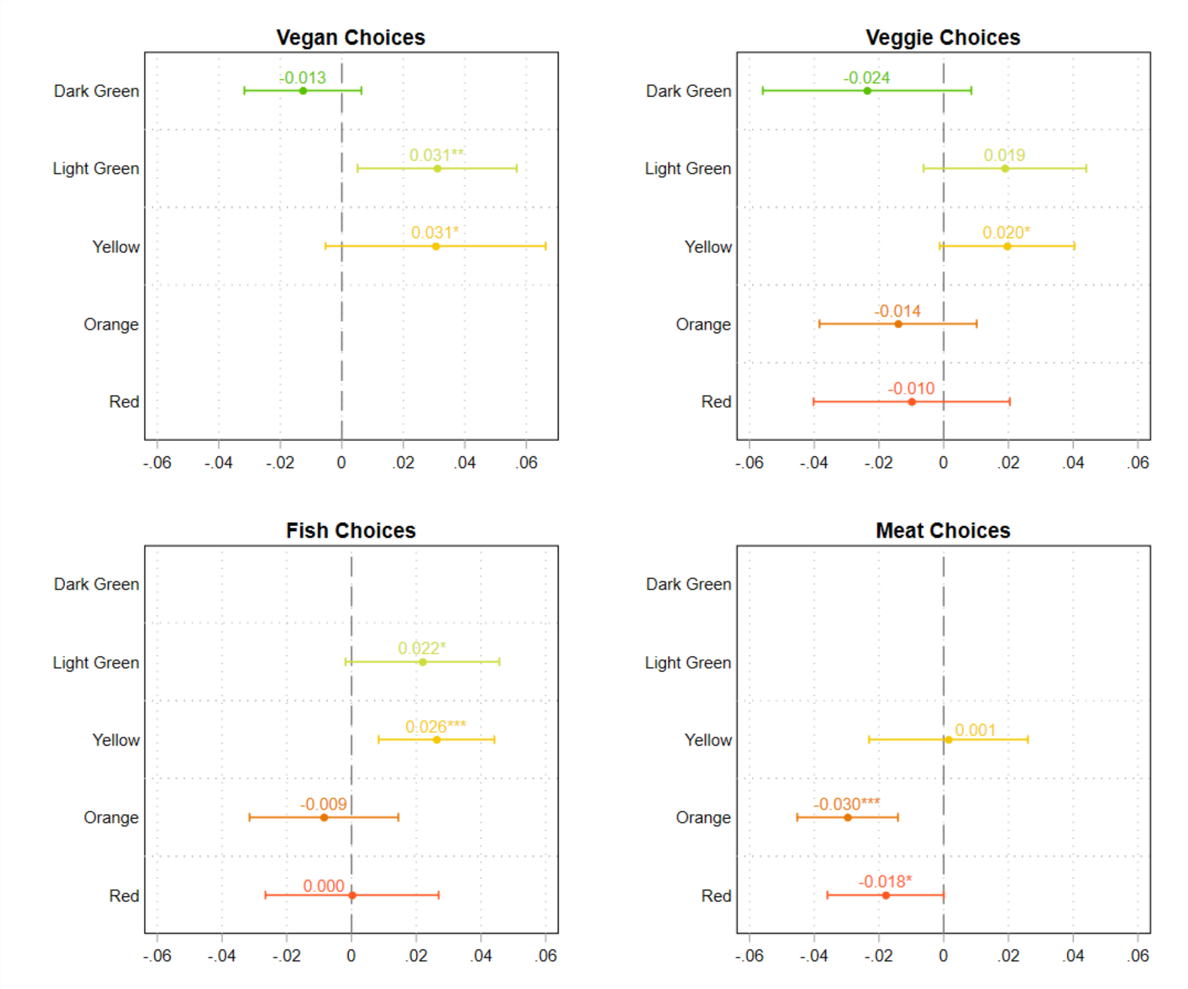

Figure 4.3.2: Marginal effects of the treatment for each label colour

Note: Mixed-logit estimates of equation (4.3) via simulated maximum likelihood estimation where the choice alternatives are defined as vegan, vegetarian, fish and meat meals. The base category is the vegetarian meal alternative which is constrained to zero. Error bars indicate 95% confidence intervals. Full model results shown in Appendix Table 4.C4.

in orange-labelled meat dishes and to a lesser extent by red-labelled meat dishes. Moreover, these findings imply that relying on aggregate measures of meat/fish to proxy sustainable food choices may lead to inaccurate conclusions, due to the diversity of carbon footprints in both categories.

4.3.6 Mechanisms

In this section we explore potential mechanisms and sources of heterogeneity driving the observed effect of carbon footprint labels on food choices. To obtain additional individual-level information to assess how carbon footprint labels were perceived by customers, we conducted an exit survey in all three treatment cafeterias after the intervention period. Specifically, we asked respondents how frequently they consulted the labels and whether the information provided by the labels was easy to understand, trustworthy, useful and important to be displayed. Additionally, we asked respondents to indicate the effect the labels had on their choices in a range of different scenarios, as well as how the labels made them feel about their meal choices. Respondents were given the option to provide their student ID numbers allowing us to link survey responses to their individual sales history. Below we will present a range of exploratory findings from a supplementary analysis using a sub-sample of observations from the sales data ($N = 8,698$) to which we were able to link exit survey responses. In total, 174 respondents who regularly dined in the cafeteria in both baseline and intervention periods provided consent for their survey responses to be linked to the sales data. It is important to note that the sample of survey respondents does not accurately represent the sample population of the full sales data. First, the sample of survey respondents is biased towards regular customers, with an average of 50 cafeteria visits over the experimental period, compared to 31 in the full sample. With respect to pre-intervention preferences, the survey subsample is biased towards individuals who already favoured low-carbon diets, with approximately 66% being in the highest two low-carbon quartiles.[21] Hence, results must be interpreted with caution. Nonetheless, the findings provide additional insights into how labels were perceived and potential mechanisms driving the observed treatment effects.

Using this sub-sample of the data, we conduct a before-after analysis regressing Y_{it} on the post-treatment indicator and an interaction of the post-treatment dummy with a selection of

[21] The surveys were administered via the college mailing lists and sent to the entire student population of each college. Participation in the surveys was optional and incentivised with a prize-draw for a £20 Amazon Voucher for each college. Survey uptake was low, and a significant proportion of individuals did not fully complete the survey. As individuals self-selected into the sample, the observed bias in diets and cafeteria visits was expected.

variables elicited in the exit survey.[22] For Y_{it} we focus on the binary variable for selecting a high carbon footprint meal, as reducing consumption of these meals holds the largest GHG mitigation potential. We estimate the marginal effect of the post-intervention variables for different levels and categories of the survey questions.

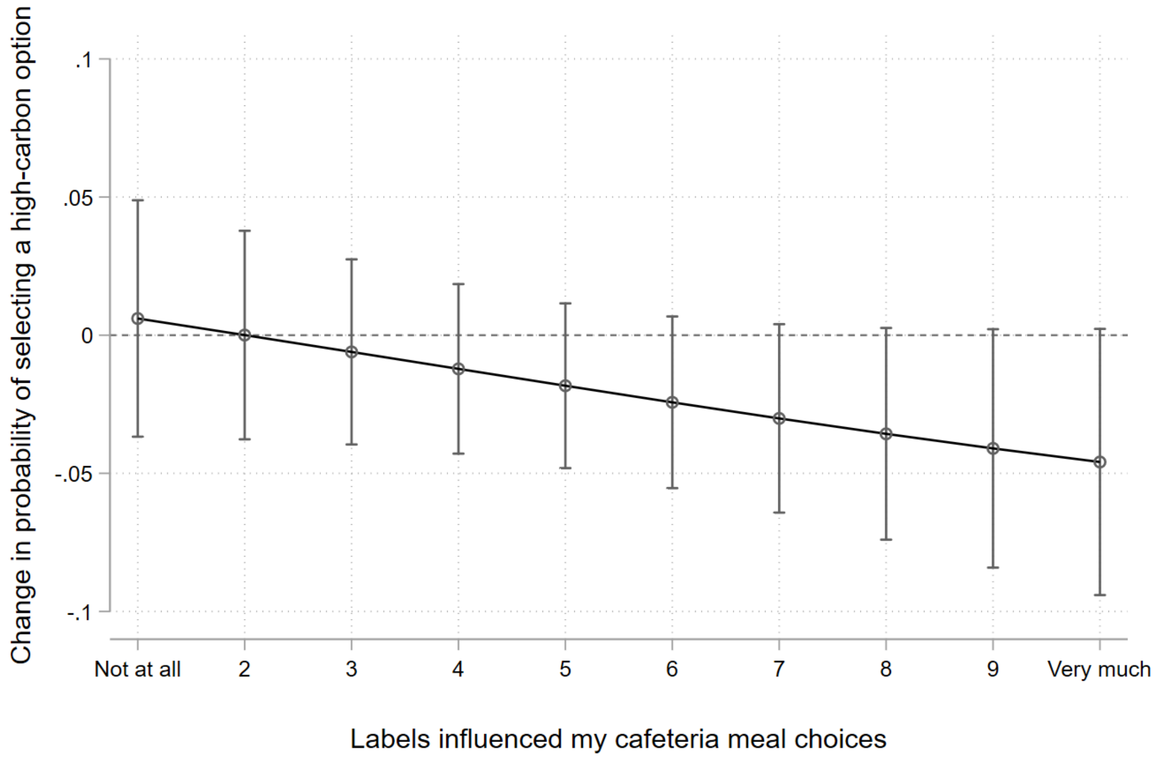

Figure 4.3.3: Change in the probability of selecting a high-carbon footprint meal by stated effect on meal choice (general influence)

Note: Error bars indicate 95% confidence intervals; $N = 7,726$, Individuals = 172.

First, we ascertain whether survey respondents replied truthfully to the survey questions by assessing the effect of self-reported influence of the labels on their meal choices. Respondents were asked to indicate, on a scale from 1 (not at all) to 10 (very much), whether the labels influenced their meal choices. The estimated marginal treatment effect is shown for all values in Figure 4.3.3.

[22] We estimate logit models for the following specification: $Y_{it} = \alpha_1 + \beta_1 S_i + \gamma_1 Post + \theta_1 Post \times S_i + X_t + \varepsilon_t$, with S_i being the survey variable of interest, X_t representing the same control variables and day-of week fixed effects specified in equation (4.1). The coefficient of interest is θ_1 capturing the post-intervention differences associated with S_i. To visualise the differences, we compute marginal effects at representative values of S_i.

Here we find a statistically significant decrease in the probability of selecting a high-carbon meal for values greater than 6 on the self-reported label influence scale (significant at the 10% level). A similar pattern emerges when interacting a variable based on the statement: Labels motivated me to choose a meal with a lower carbon footprint (see Appendix Figure 4.D1). Taken together these results support the internal consistency between the exit survey responses and actual observed meal choice behaviour.

Next, we explore avoidance behaviour and general perceptions towards the labels. To assess potential avoidance behaviour and attention towards the labels, we asked survey respondents how often, if at all, they saw and read the labels. Appendix Figure 4.D2 shows that the majority of individuals paid attention to the labels on most occasions (31% most meals, 30% always). However, 12% stated to have never seen or read the labels. To assess general perceptions, we asked respondents to indicate, on a scale from 1 (not at all) to 10 (very much), how easy to understand, trustworthy/reliable, useful and important the information provided by the labels was. Appendix Figure 4.D3 shows that overall, the labels were well received. The majority of individuals believed that the information was easy to understand and important to be displayed (M = 9), as well as trustworthy (M = 7) and useful (M = 8). We hypothesised that differences in attention and perceptions may influence the effectiveness of the labels in reducing high carbon meal choices, however, logit regressions show no statistically significant difference for both attention and perceptions.

We further hypothesised that a potential mechanism driving the efficacy of labels in changing behaviour could be the emotional response to the labels (Schneider et al., 2021; Taufik, 2018; Thunström, 2019). 'Warm Glow', the positive emotional reward from acting pro-socially, has recently received increasing attention as an important motive for meat reduction (Taufik, 2018) and pro-environmental behaviour in general (van der Linden, 2018). To measure experienced positive and negative emotions, we asked survey participants to indicate how the labels made them feel about their meal choices, using the Qualtrics graphic "smile" slider, ranging from 1 (very unhappy) to 5 (very happy). In Figure 4.3.4 we plot the marginal treatment effect of the labels on high-carbon meal choices for each of the five categories of emotional response.

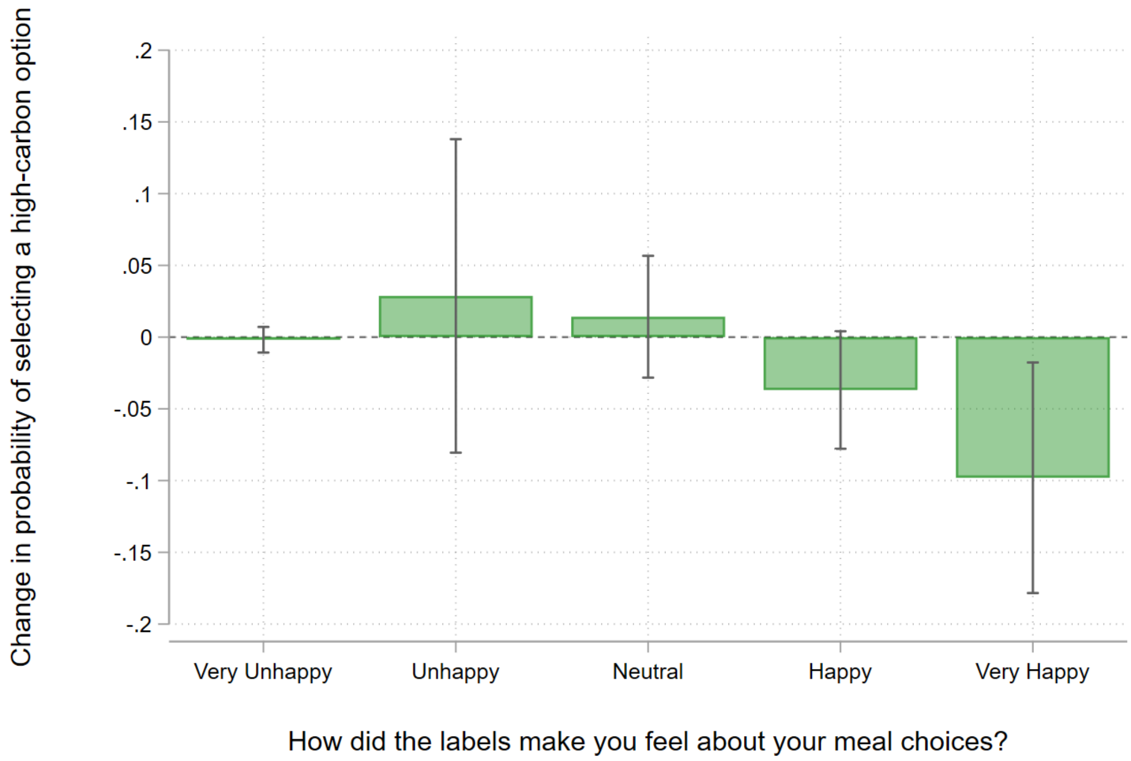

Figure 4.3.4: Difference in treatment effect by emotional response to the labels

Note: Error bars indicate 95% confidence intervals; $N = 7,409$, Individuals = 167.

The results show that only those individuals who experienced positive emotions in relation to the labels decreased their consumption of high-carbon meals (significant at the 10% and 5% levels, respectively). Moreover, respondents who reported feeling very happy reduced their consumption of high carbon meals to a greater extent (6 percentage points) than individuals who felt happy, however, the difference is not statistically significant at conventional levels (z = 0.18). Nonetheless, the findings presented in Figure 4.3.4 suggests that 'warm glow' may be a potential mechanism through which labels encourage more sustainable carbon choices.

4.4 Discussion and conclusion

Consumers are becoming increasingly aware of the environmental impact of their food consumption. Whilst taste, price and quality remain the most important determinants of food choices, there is a growing share of the population that holds climatarian preferences and actively pursues diets that aim to reduce their environmental impact. According to recent consumer research by the Carbon Trust, almost two-thirds of consumers say they would feel more positive towards companies that have reduced the carbon impact of their products.[23] However, a lack of consumer knowledge on the carbon footprint and environmental impact of foods remains a significant barrier for individuals to align their actions with their pro-environmental values.

Carbon footprint labels address the market failure of information asymmetry and aim to bring about sustainable behaviour change in many consumer domains, given sufficient demand for low-carbon alternatives (Vandenbergh et al., 2011). This chapter reports findings from a large-scale field experiment in a real-world cafeteria setting, measuring the impact of carbon labels on meal choices and consumer demand for lower-carbon meal options. We find that carbon footprint labels have statistically significant impacts on meal choices. Specifically, we show that labels shifted consumer choices from high-carbon impact meals to mid-carbon impact meals by 2.7 percentage points and decreased sales of meat and fish dishes by 1.6 percentage points (with an equivalent increase in vegan/vegetarian dishes). Effects are most pronounced for individuals who followed a high-carbon footprint diet prior to the intervention. While the effect sizes appear modest, they are highly statistically significant and robust across numerous alternative specifications, providing clear evidence that climatarian preferences exist in our sample population which can be leveraged by providing carbon footprint information.

How do carbon footprint labels compare to frequently used behavioural nudges designed to encourage sustainable diets? To allow for meaningful comparisons, we compare our estimates with findings from other behavioural interventions conducted in similar cafeteria settings. For instance, Kurz (2018) shows that changing the menu order and salience of the vegetarian meal option increased the probability of selecting a vegetarian meal by 6 percentage points. Similarly, Garnett et al. (2020) find that changing the order of vegetarian and meat options, by placing the vegetarian option first, increased the probability of selecting vegetarian options by on average 5.4 percentage points if choices were placed more than 1.5 meters apart. Taken together, the findings from this literature suggest that nudges have a more pronounced impact on food choices than carbon labels do. In contrast to behavioural interventions, however, carbon footprint labels serve as an information instrument, which can affect both the salience of information at the time of purchase and consumer awareness and knowledge. While the effects of nudges appear to be more impactful, they might also be more short-lived (e.g. Allcott & Rogers, 2014). In contrast, labels have the potential to have a sustained long-run impact on consumers by gradually building a stock of knowledge with

[23] See Carbon Trust, Consumer Research 2020: Product carbon footprint labelling. Accessed from https://www.carbontrust.com/resources/product-carbon-footprint-labelling-consumer-research-2020 [October, 2021]

respect to the carbon footprint of food. To that end, it may require a considerable amount of time for new information to "sink in" before it starts to consistently impact decision-making and may eventually also spill-over into other food choice contexts. While there exist few long-term evaluations, Thorndike et al. (2014) provide results from a 2-year trial on the effectiveness of a food labelling intervention to promote healthier choices in a cafeteria setting and find that the intervention led to sustained improvements in healthy food and beverage choices. Therefore, our results, although comparably small, support the viability of labels as a complementary policy instrument. More research is required to establish the long-term effects of carbon-footprint labels.

In addition, it is important to point out that the previously discussed behavioural interventions were specifically designed to nudge consumers towards choosing vegetarian dishes. However, the primary intention of carbon footprint labels is to encourage more sustainable choices, regardless of whether the lower-carbon alternative is a vegetarian, fish or meat meal. Our analysis in Section 4.3.5 shows that labels have much more nuanced effects on food choices which are not accurately captured by aggregate measures of vegan/vegetarian and fish/meat meal choices. For instance, we find that labels decreased the probability of selecting the highest carbon meat alternatives (orange and red labelled) but had no effect on mid-carbon meat dishes and even increased the sales of light-green and yellow-labelled fish dishes. These promising findings suggest that consumers indeed respond to carbon footprint information regardless of which type of meal is being offered, yet it significantly complicates a direct comparison of labels to the previously discussed behavioural interventions. More generally, we acknowledge that it is difficult to compare treatment intervention effect sizes from different studies, conducted in different contexts and experimental settings. Further research is needed to implement direct experimental testing, in the form of multi-treatment studies, to provide truly comparable estimates for different policy instruments.

When assessing the efficacy of different policy tools for sustainable behaviour change, including information provision, it is important to also evaluate welfare effects (Sunstein, 2021). So far, only few studies have considered welfare effects of labels (and nudges) and results from these welfare evaluations are mixed (Allcott & Kessler, 2019; Bulte et al., 2020; Damgaard & Gravert, 2018; Ho et al., 2021; Thunström, 2019). For instance, Thunström (2019) suggest that calorie labels on restaurant menus pose an "emotional tax" on some individuals, while she also ascertains considerable heterogeneity in the emotional response to the label.[24] Our supplementary analysis using survey data provides tentative evidence that emotions may be a

[24] Relatedly, people may actively avoid food product information if it imposes hedonic costs (Edenbrandt et al., 2021; Reisch et al., 2021; Sunstein, 2019)

key pathway through which labels affect consumer choices. We find that labels only changed behaviour for those individuals who reported that the labels made them feel ‘happy’ or ‘very happy’ and had no effect on individuals who felt ‘neutral’ or ‘unhappy’. These results need to be interpreted with caution, due to the small and self-selected sample in the exit survey. Nonetheless, the survey data reveals that 7% of individuals reported that the labels made them feel ‘unhappy’ or ‘very unhappy’ which indicates that the net-benefits of carbon footprint labels may be lower, after accounting for such emotional costs. More research is needed to assess whether carbon labels may have other unintended consequences and potential negative welfare effects, which could undermine their policy relevance.

A further key marker to judge the efficacy of carbon footprint labels as a policy tool is to evaluate the overall emissions mitigation potential. Our data allow us to directly estimate the average emissions reduction per 100g serving. Using the observed carbon footprint of meal choices as the dependent variable in our main specification, we estimate a direct treatment effect of 27g CO_2 (or 4.3%) reduction per 100g serving. This value is in line, yet slightly larger than previous findings from the labelling literature.[25] Although the reduction in emissions may appear modest, a reduction of 27g CO_2 (or 4.3%) per 100g serving should not be understated. For our intervention period, in which about 26,000 meals were sold in the treatment cafeterias, each with an average footprint of 2 kg CO_2e, the labelling intervention thus led to a reduction of 2.21 tons of CO_2. If we scale up this estimate for all 31 college cafeterias in Cambridge for a typical term time of 8-weeks with a total of roughly 350,000 meals sold (accounting for different college sizes), this could lead to savings of approximately 30 tonnes of CO_2 per term.

Conducting a simple back-of-the-envelope calculation for a typical university cafeteria, we obtain the following estimates for the costs of avoided CO_2 emissions from our labelling intervention. Given that in our university cafeteria setting, a label intervention is able to avert 4.3% of carbon emissions of every meal with an average impact of 2 kg CO_2e per serving, we scale up this point estimate to a representative university cafeteria which serves 1000 meals per day. Doing so results in savings of 86 kg CO_2/day, which is about 2.5 tonnes per month. Based on pricing estimates by Foodsteps Inc. the average cost faced by the cafeteria for implementing the label programme on all meals using professional carbon footprinting and labelling software amounts to approximately £80/month. We treat this amount as the programme cost, however,

[25] For instance, Muller et al. (2019) find that labels induced a reduction between 14 and 19g per 100g for a basket of goods in an experimental online store setting. Brunner et al. (2018) estimate that the carbon footprint labelling scheme at a university restaurant reduced overall GHG emissions by 3.6%.

there may be minor personnel costs for administering the programme (e.g., printing the labels). For one month, the total abatement cost is thus £31 per tCO_2 emissions avoided.

To summarise, our results suggest that labels are an effective tool to leverage pro-environmental preferences in a cafeteria setting and promise considerable GHG emission reductions at the individual level. Whilst our study is limited to the cafeteria setting, carbon labels will have a much larger role to play in a broader set of food consumer choices, in particular in supermarket purchase decisions (the volume of which is much larger than cafeteria choices). Additional experiments in these food choice settings with non-student samples will be important to solidify our understanding of how carbon footprint labels affect consumer choices. We leave this to future research.

Moreover, labels allow for product differentiation on sustainability grounds and hence provide clear signals to consumers who hold environmental preferences. Product differentiation aids consumer choices and in turn may bring about significant changes on the producer side if market dynamics continue on their current trend in favour of low-carbon alternatives and increasing climatarian dietary preferences. For instance, labels may incentivise suppliers to substitute high-carbon alternatives in favour of lower-carbon alternatives, which could result in substantial decreases in food production emissions. If future carbon footprint labels are based on full life-cycle assessments capturing emissions from 'farm to fork', this could further encourage innovations along the entire supply chain. Labels in other areas have in the past proven to set industry standards, such as the 'GMO-Free' labels and animal welfare labels (e.g., dolphin-safe tuna). In both cases, labels were introduced to achieve product differentiation capitalising on changes in consumer preferences which eventually became the industry standard.

Our study and results are particularly relevant under the current policy climate in the UK, the EU and elsewhere where pilot voluntary carbon food labelling schemes are emerging (e.g. the UK's Carbon Trust label) and advanced discussions are underway for introducing carbon food labels as part of many countries' decarbonisation agendas. This momentum is partly a reaction to an increasing consumer shift towards climatarian diets (i.e. diets aimed at reducing the carbon footprint). Yet, the reality remains that rolling out carbon food labels across the entire food industry is an immensely challenging and complex endeavour, while at the same time, causal hard evidence-based studies on the impact of these labels on actual behaviour are lacking (Rondoni & Grasso, 2021). This chapter provides one of the first large-scale field experiments specifically assessing these impacts in a causal manner. We find that carbon footprint labels on food could induce carbon reducing behavioural changes. The challenges that remain are how to scale up the use of such labels in a manner that is unambiguous to consumers and also cost-effective.

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Appendix

Appendix 4.A Experimental design and robustness

Table 4.A1: Menu Composition across treatment and control cafeterias over the experimental period

	Treatment cafeterias				Control Cafeterias			
Menu Composition	Baseline		Intervention		Baseline		Intervention	
Low-Carbon	0.37	(0.14)	0.38	(0.16)	0.39	(0.17)	0.36	(0.15)
Mid-Carbon	0.33	(0.12)	0.33	(0.12)	0.27	(0.10)	0.25	(0.08)
High-Carbon	0.46	(0.17)	0.46	(0.16)	0.47	(0.18)	0.48	(0.18)
Meat/Fish	0.54	(0.07)	0.53	(0.07)	0.54	(0.07)	0.53	(0.07)
Vegan/Vegetarian	0.46	(0.07)	0.47	(0.07)	0.46	(0.07)	0.47	(0.07)
Carbon Footprint	540.13	(213.65)	532.02	(224.36)	570.65	(226.04)	586.90	(252.24)

Note: Table shows the percentage of meals *available* for each dependent variable during the entire baseline and intervention period in both treatment and control cafeterias. "Low-Carbon" includes meals labelled dark and light green; "Mid-Carbon" includes yellow labelled meals and "High-Carbon" includes orange and red labelled meals (see Figure 4.2.1). Carbon Footprint in grams of CO_2 equivalent per 100g serving. Meal shares are computed based on observations where the exact meal choice could be identified as one of multiple alternatives for each respective dependent variable (i.e., excluding observations where the dependent variable was not available). For this reason, the sum of menu compositions for low, mid and high-carbon alternatives is greater than 1. Standard deviation in parentheses.

Table 4.A2: Robustness: Main results excluding controls

	Climatarian Preferences				
	(1)	(2)	(3)	(4)	(5)
	Low	Mid	High	GHG	Fish/Meat
Post × Treat	0.026***	0.047***	-0.030***	-28.984***	-0.018**
	(0.008)	(0.009)	(0.008)	(8.919)	(0.007)
ID & Week FE	Yes	Yes	Yes	Yes	Yes
Individuals	2,005	1,899	2,014	2,043	2,672
Observations	58,006	39,672	58,612	61,239	78,393

Note: OLS estimates of equation (4.1), excluding all time-varying control variables. The dependent variables in columns (1)-(3) are indicators for low, mid and high-carbon meal choice, respectively. The dependent variable in column (4) is a continuous variable for the carbon footprint of meal choice. The dependent variable in column (5) is an indicator for fish/meat meal choice. Post × Treat is the difference-in-differences estimator (δ^{DD}) capturing the treatment effect. Controls include total sales, total number of options available, number of options available for Y, price differential between veg and meat or high-carbon and low-carbon alternatives, indicator for dinner service, hourly temperature and day-of-week dummies. All models include individual and week fixed effects. Standard errors clustered at the individual level in parentheses.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Table 4.A3: Robustness: Clustering

		P-value	
	δ^{DD}	Cluster ID	Cluster Cafeteria
	(1)	(2)	(3)
Low	0.003	0.730	0.880
Mid	0.027	0.003	0.071
High	-0.027	0.001	0.097
Footprint	-26.786	0.002	0.170
Meat/Fish	-0.016	0.018	0.025

Note: Table shows p-values calculated under various clustering regimes. Coefficients (δ^{DD}) obtained from equation (4.1) and p-values from our preferred clustering approach (Cluster ID) are shown in Columns (1) & (2), respectively. Column (3) clusters at the cafeteria-level and P-values are calculated using the wild bootstrap-t method (Roodman et al., 2019)

Table 4.A4: Robustness: Rebound effects and attrition

	(1) Total Sales	(2) Least Green	(3) Less Green	(4) More Green	(5) Most Green
Post × Treat	7.113	-0.005	-0.005	0.001	0.008
	(4.729)	(0.011)	(0.008)	(0.007)	(0.010)
Mean Dep. Var	90.032	0.256	0.274	0.250	0.221
Observations	941	727	727	727	727

Note: Table shows the average treatment effect of the labelling intervention on total sales (Column 1) and on the share of customers from each pre-intervention preference group (Columns 2 - 5), classified by baseline low-carbon meal consumption. Sales data are aggregated at the 'cafeteria-service' level. Robust standard errors in parentheses.

Appendix 4.B Pre-trends assessment

Table 4.B1: Test for equality of pre-intervention trends between treatment and control groups

	(1) Low	(2) Mid	(3) High	(4) Footprint	(5) Veg
Treat × Time-Trend	-0.00006	0.00044	-0.00019	0.41514	-0.00013
	(0.23)	(0.42)	(0.35)	(0.19)	(0.53)
Obs.	36,111	24,406	37,029	38,415	49,637

Note: Estimates obtained from separate linear regressions with individual fixed effects of each dependent variable on a linear time trend (number of cafeteria services since 7th October 2019), an interaction of the time trend with the treatment indicator and all other control variables specified in equation (4.1) using pre-intervention sales data (7th October 2019 - 26th January 2020). The coefficient reported in each column is the estimate from the interaction of the time trend with the treatment indicator. Standard errors are clustered at the cafeteria level using the wild bootstrap-*t* method (Roodman et al., 2019). Wild bootstrap-*t* p-values are shown in parenthesis.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

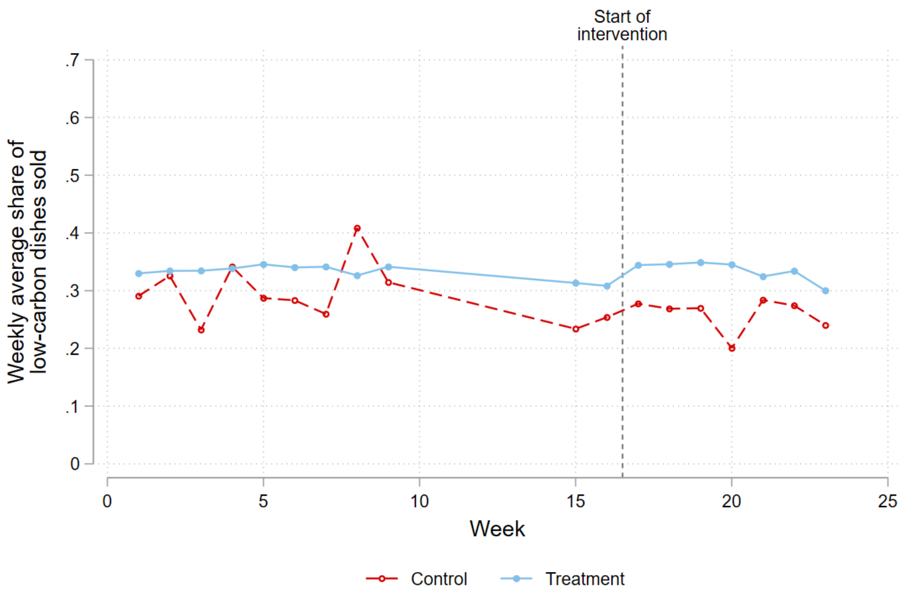

Figure 4.B1: Weekly average sales share of low-carbon footprint dishes; *Note*: Based on $N = 58,006$ individual sales.

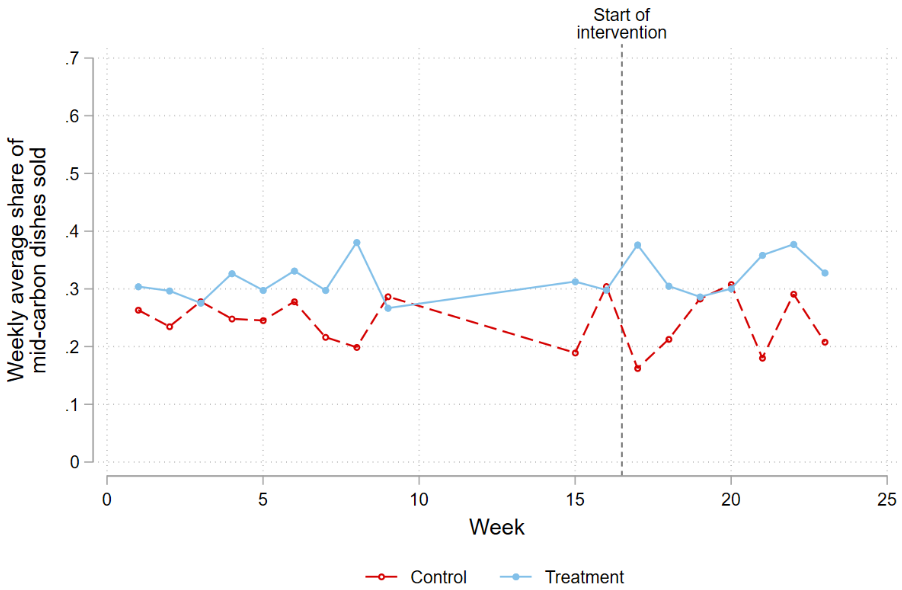

Figure 4.B2: Weekly average sales share of mid-carbon footprint dishes; *Note*: Based on $N = 39,672$ individual sales.

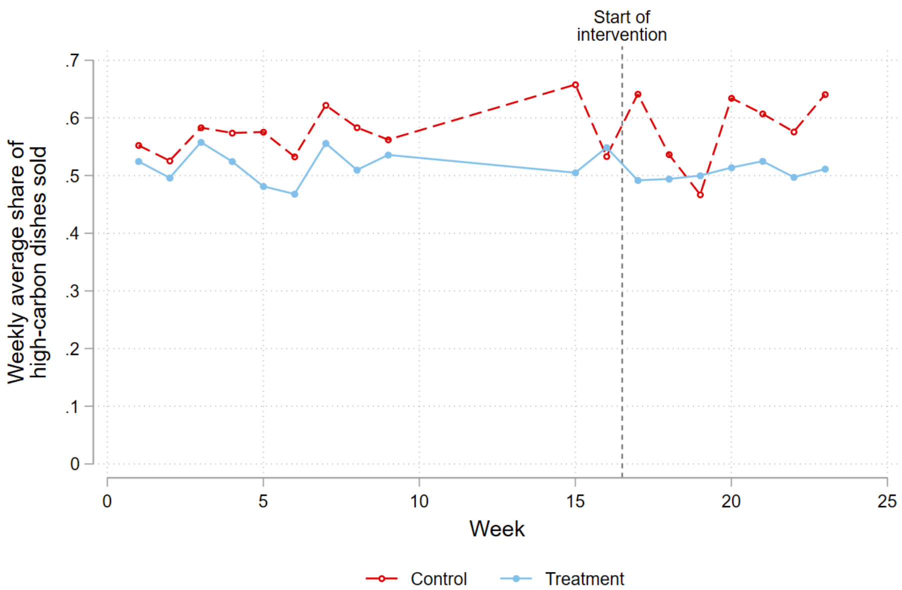

Figure 4.B3: Weekly average sales share of high-carbon footprint dishes; *Note*: Based on $N = 58,612$ individual sales.

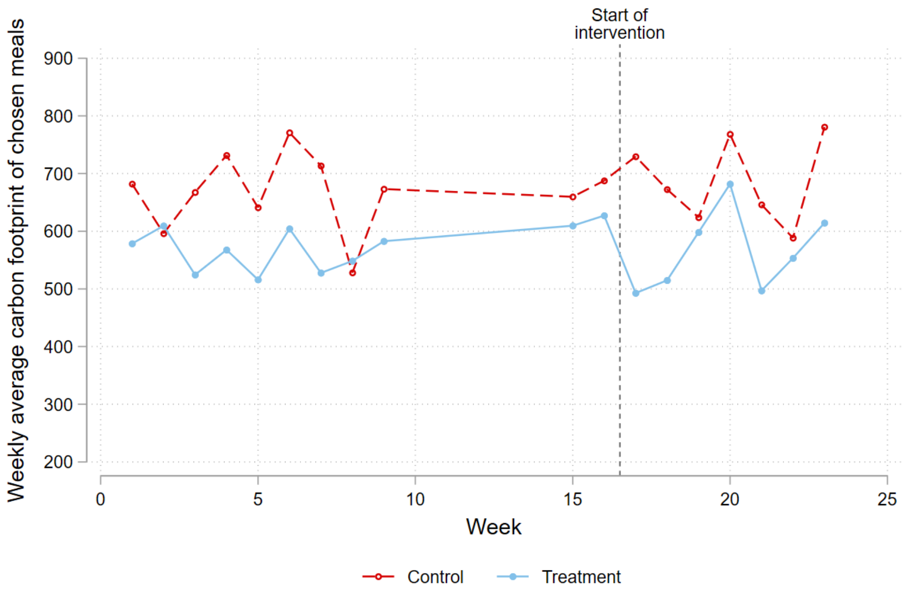

Figure 4.B4: Weekly average carbon footprint of chosen meals; *Note*: Based on $N = 61,239$ individual sales.

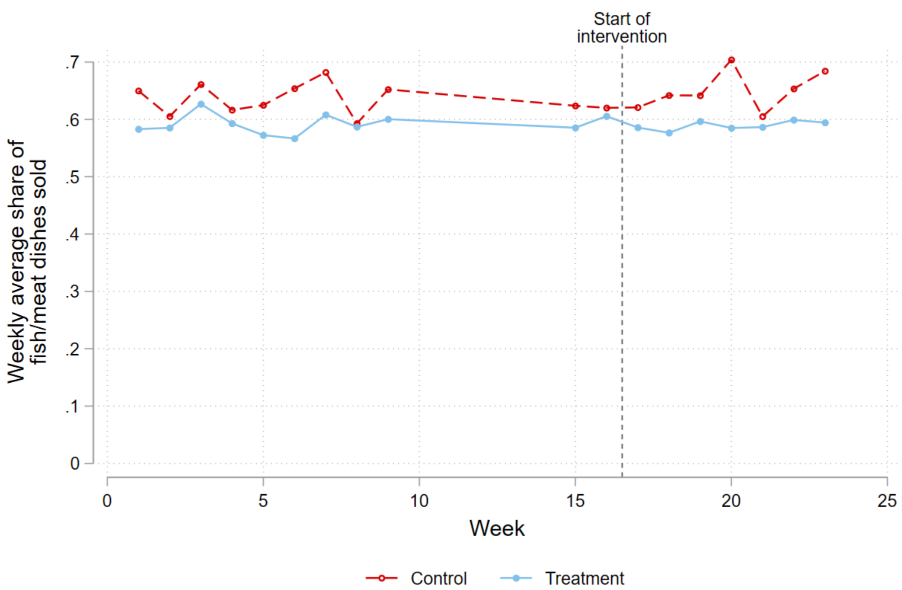

Figure 4.B5: Weekly average sales share of meat/fish dishes sold; *Note*: Based on $N = 78,393$ individual sales.

Appendix 4.C Full regression tables

Table 4.C1: Full results table: OLS estimates

	Climatarian Preferences				
	(1) Low	(2) Mid	(3) High	(4) GHG	(5) Fish/Meat
Post × Treat	0.003	0.027***	-0.027***	-26.786***	-0.016**
	(0.008)	(0.009)	(0.008)	(8.800)	(0.007)
Price Diff. Low/High	0.036***	-0.031***	-0.016*		
	(0.009)	(0.010)	(0.009)		
Nr. Low Options	0.185***				
	(0.004)				
Nr. Mid Options		0.227***			
		(0.006)			
Nr. High Options			0.185***		
			(0.004)		
Price Diff. Veg/Meat					-0.027**
					(0.011)
Nr. Fish/Meat Options					0.094***
					(0.005)
Nr. Meal Options	-0.055***	-0.069***	-0.096***	43.566***	-0.056***
	(0.003)	(0.004)	(0.004)	(4.286)	(0.004)
Total Sales	0.000***	-0.000**	-0.000	-0.743***	0.000***
	(0.000)	(0.000)	(0.000)	(0.074)	(0.000)
Dinner Service	-0.005	0.019**	-0.003	42.767***	-0.017***
	(0.006)	(0.008)	(0.007)	(8.662)	(0.005)
Average Temperature	0.001	0.002*	-0.002**	1.049	-0.003***
	(0.001)	(0.001)	(0.001)	(0.999)	(0.001)
Monday	0.102***	0.066***	-0.111***	-249.627***	-0.103***
	(0.015)	(0.019)	(0.015)	(27.039)	(0.012)
Tuesday	0.064***	0.059***	-0.069***	-156.282***	-0.048***
	(0.013)	(0.020)	(0.014)	(26.067)	(0.010)
Wednesday	0.062***	0.058***	-0.078***	-169.465***	-0.094***
	(0.014)	(0.019)	(0.014)	(26.713)	(0.011)
Thursday	0.045***	0.052***	-0.068***	-7.295	-0.075***
	(0.013)	(0.019)	(0.014)	(26.630)	(0.010)
Friday	0.081***	0.171***	-0.200***	-238.396***	-0.020*
	(0.014)	(0.020)	(0.015)	(26.513)	(0.011)
Saturday	0.083***	0.073***	-0.124***	67.838**	-0.030***
	(0.014)	(0.022)	(0.016)	(29.928)	(0.011)
Constant	0.163***	0.215***	0.688***	631.544***	0.725***
	(0.022)	(0.030)	(0.023)	(33.174)	(0.020)
ID & Week FE	Yes	Yes	Yes	Yes	Yes
Individuals	2,005	1,899	2,014	2,043	2,672
Observations	58,006	39,672	58,612	61,239	78,393

Note: OLS estimates of equation (4.1). The dependent variables in Columns (1)-(3) are binary indicators equal to one if a low, mid or high-carbon meal option was chosen, respectively, and zero if any other alternative was chosen. The dependent variable in column (4) is a continuous variable for the carbon footprint of meal choice. The dependent variable in column (5) is an indicator for fish/meat meal choice. Post × Treat is the difference-in-differences estimator (δ^{DD}) capturing the treatment effect. All models include individual and week fixed effects (omitted from output). Standard errors clustered at the individual level in parentheses.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Table 4.C2: Climatarian choices: Mixed-logit estimates

	(1) Mixed-Logit	
Price	-0.375***	(0.067)
Nr. Options	0.688***	(0.010)
Low	-0.369*	(0.198)
Mid	0.000	(.)
High	1.728***	(0.183)
Random parameters		
σLow	0.812***	(0.028)
$Corr(Low, High)$	-0.449***	(0.037)
$\sigma High$	1.012***	(0.029)
$\sigma Price$	2.212***	(0.071)
Alternative: Low		
Post	-0.047	(0.049)
Treat	0.252***	(0.069)
Post × Treat	-0.033	(0.062)
Alternative: High		
Post	0.017	(0.044)
Treat	-0.018	(0.071)
Post × Treat	-0.158***	(0.058)
Observations	157,184	
Nr. Cases	61,395	
Individuals	2,331	

Note: Mixed-logit estimates of equation (4.2) via simulated maximum likelihood estimation where the choice alternatives are defined as low, mid and high carbon meals. The base category is the mid-carbon meal alternative which is constrained to zero. Additional case-specific covariates include controls for total sales, total number of options available, average hourly temperature, an indicator for dinner service and day-of-week dummies (omitted from output). Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.C3: Vegetarian choices: Mixed-logit estimates

	(1) Mixed-Logit	
Price	-0.320***	(0.081)
Nr. Options	0.359***	(0.015)
Meat/Fish	0.000	(.)
Vegetarian/Vegan	-1.494***	(0.133)
Random parameters		
$\sigma Price$	-1.155***	(0.107)
σVeg	2.069***	(0.048)
Alternative: Veg		
Post	-0.060**	(0.029)
Treat	0.251***	(0.097)
Post × Treat	0.118***	(0.042)
Observations	157,470	
Nr. Cases	78,735	
Individuals	3,014	

Note: Mixed-logit estimates of equation (4.2) via simulated maximum likelihood estimation where the choice alternatives are defined as vegan/vegetarian and meat/fish meals. The base category is the meat/fish alternative which is constrained to zero. Additional case-specific covariates include controls for total sales, total number of options available, average hourly temperature, an indicator for dinner service and day-of-week dummies (omitted from output). Standard errors clustered at the individual level in parentheses.

* p < 0.1, ** p < 0.05, *** p < 0.01.

Table 4.C4: Meal type by label colour: Mixed-logit estimates

	(1) Mixed-Logit	
Price	-0.147**	(0.062)
Nr. Options	0.666***	(0.015)
Dark-Green	-0.256***	(0.063)
Light-Green	-0.031	(0.045)
Yellow	0.000	(.)
Orange	0.143***	(0.048)
Red	0.106*	(0.058)
Vegan	-0.734***	(0.193)
Vegetarian	0.000	(.)
Fish	-0.586*	(0.308)
Meat	0.842***	(0.195)
Random parameters		
$\sigma Price$	1.081***	(0.094)
$\sigma Vegan$	-0.772***	(0.026)
$\sigma Vegetarian$	0.000	(.)
$\sigma Fish$	1.488***	(0.047)
$\sigma Meat$	2.043***	(0.060)
Alternative: Vegan		
Post	-0.079	(0.080)
Treat	0.381***	(0.083)
Post × Treat	0.064	(0.095)
Alternative: Fish		
Post	-0.039	(0.055)
Treat	-0.071	(0.090)
Post × Treat	0.050	(0.075)
Alternative: Meat		
Post	-0.159**	(0.065)
Treat	0.045	(0.116)
Post × Treat	-0.045	(0.085)
Observations	198,481	
Nr. Cases	61,526	
Individuals	2,331	

Note: Mixed-logit estimates of equation (4.3) via simulated maximum likelihood estimation where the choice alternatives are defined as vegan, vegetarian, fish and meat meals. The base category is the vegetarian meal alternative which is constrained to zero. Additional alternative-specific variables include interactions ($label^J \times T$), ($label^J \times P$) and ($label^J \times T \times P$) which are omitted from the output for readability purposes. Additional case-specific covariates include controls for total sales, total number of options available, average hourly temperature, an indicator for dinner service and day-of-week dummies (omitted from output). Standard errors clustered at the individual level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 4.D Exit survey results

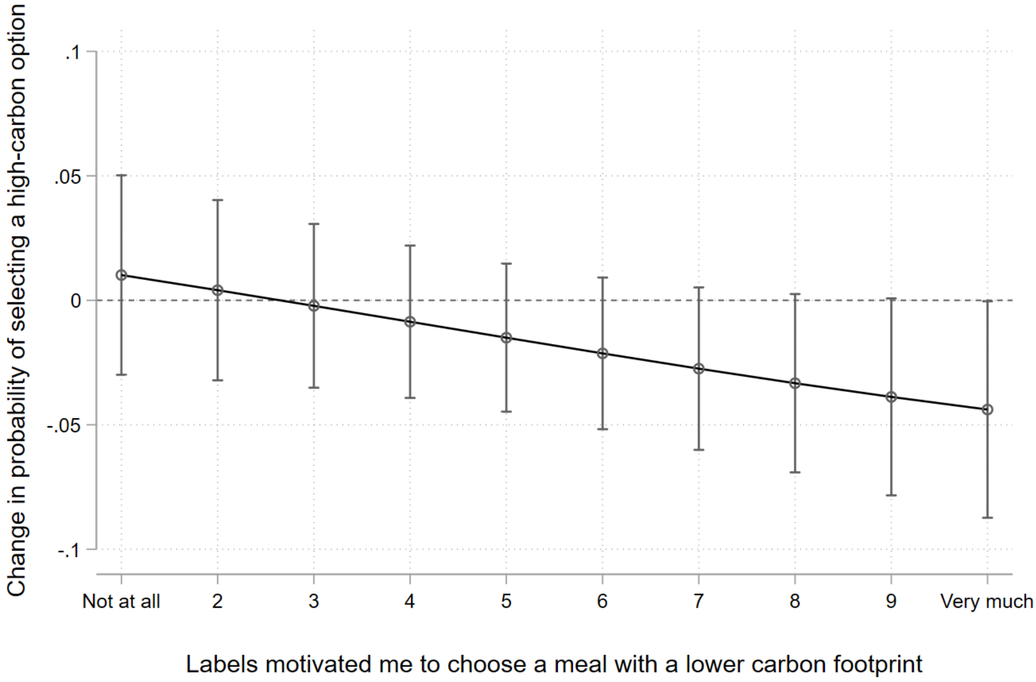

Figure 4.D1: Treatment effect on probability of selecting a high-carbon footprint meal by stated effect on meal choice (choose lower carbon meal)

Note: Error bars indicate 95% confidence intervals, $N = 7,726$, Individuals = 172.

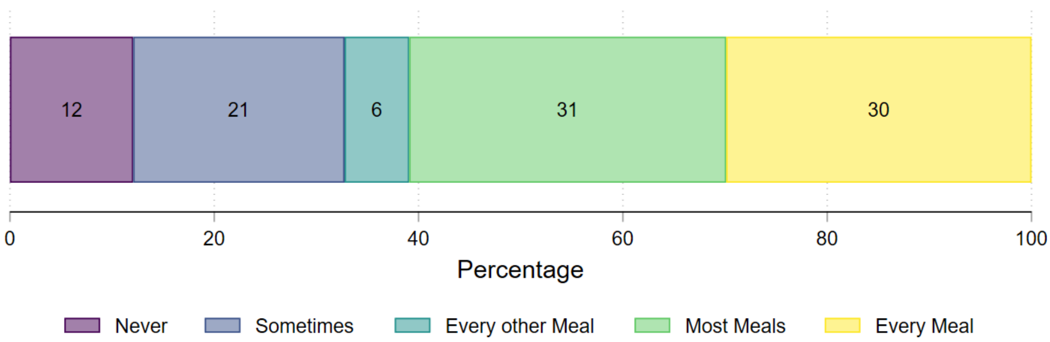

Figure 4.D2: Self-reported attention to labels

Note: Based on survey question: How often did you read/notice the labels? ($N = 174$).

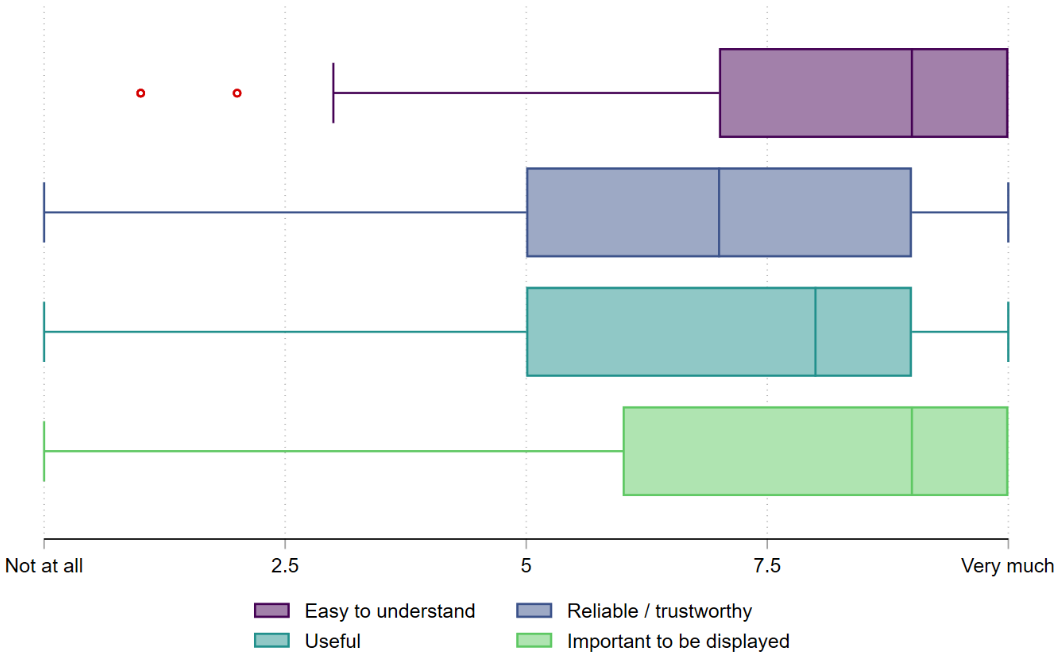

Figure 4.D3: Self-reported perception of labels

Note: Based on survey question: In your view, the information provided by the carbon footprint labels was: (1) easy to understand, (2) reliable/trustworthy, (3) useful, (4) important to be displayed. Vertical lines indicate the median, boxes indicate 25th to 75th percentiles, error bars indicate lower and upper adjacent values, dots represent outliers. (N=174).

Appendix 4.E Carbon Footprint Calculation Methodology

Beef Chilli Burger

Supplier:

Yield: 60 portions

Ingredients:

Ingredients	Quantity	
Sesame Seeded Burger Buns	60 each	
Beef Burgers	60 each	
Chilli Cheese	1 Block	
Tomatoes	60 Slices	
Lettuce Leaves	1 each	
Salt & Pepper		
Smoked Paprika	3 tbsp	
Burger Relish	½ Tin	

Figure 4.E1: Sample recipe

	recipe	ingredient_name	lca_match	region	qty	unit	ghg_emis_pp	ghg
1	BEEF CHILLI BURGER	QUARTER POUND BEEF BURGERS	Beef	British	.113	kilogram	37.12	4.19456
2	BEEF CHILLI BURGER	CHILLI CHEESE	Cheese	European	.0241667	kilogram	15.95	.3854584
3	BEEF CHILLI BURGER	WHEAT FLOUR	Wheat and rye	European	.0337225	kilogram	1.62	.0546305
4	BEEF CHILLI BURGER	TOMATO PASTE	Tomatoes	European	.0057402	kilogram	8.03	.0460935
5	BEEF CHILLI BURGER	TOMATOES	Tomatoes	European	.0133333	kilogram	1.1	.0146667
6	BEEF CHILLI BURGER	SUGAR	Sugar	Global	.0037033	kilogram	3.2	.0118507
7	BEEF CHILLI BURGER	GREEN SALAD LEAVES	Lettuce, goards and cucumbers	European	.01	kilogram	.81	.0081
8	BEEF CHILLI BURGER	RAPESEED OIL	Oil (rapeseed)	European	.0012198	kilogram	3.48	.0042447
9	BEEF CHILLI BURGER	SMOKED PAPRIKA	Tomatoes	European	.0004082	kilogram	7.7	.0031435
10	BEEF CHILLI BURGER	GHERKINS	Lettuce, goards and cucumbers	European	.0020368	kilogram	1.53	.0031164
11	BEEF CHILLI BURGER	VINEGAR	Wine	European	.0012962	kilogram	1.94	.0025146
12	BEEF CHILLI BURGER	PEPPERS RED	Tomatoes	Global	.001111	kilogram	2.09	.002322
13	BEEF CHILLI BURGER	SUGAR	Sugar	Global	.0007175	kilogram	3.2	.002296
14	BEEF CHILLI BURGER	TOMATOES (MM)	Tomatoes	European	.0016665	kilogram	1.1	.0018332
15	BEEF CHILLI BURGER	ONIONS WHITE	Onions and leeks	European	.002222	kilogram	.48	.0010666
16	BEEF CHILLI BURGER	CARROTS	Root vegetables	European	.0003703	kilogram	.42	.0001555
17	BEEF CHILLI BURGER	WATER			.	kilogram	.	.
18	BEEF CHILLI BURGER	YEAST			.	kilogram	.	.

Figure 4.E2: Sample recipe breakdown

Note: `lca_match` refers to the matched LCA parent class. `qty` refers to the weight of the raw ingredient in kilograms per serving. `ghg_emis_pp` are the matched LCA values (in kilograms) with packaging and processing penalties applied. `ghg` are the emissions per serving for each ingredient (`qty` × `ghg_emis_pp`).

	recipe	ghg	qty	ghg100	type	label_colour
1	RED LENTIL & CHICKPEA TAGINE	.3348133	.4903333	68.2828	Vegan	Dark-Green
2	SWEET POATO & SPINACH MADRAS	.23435	.276	84.90942	Vegan	Dark-Green
3	MUSHROOM, LENTIL & WALNUT RAGU	.5423316	.5824888	93.10593	Vegetarian	Dark-Green
4	MUSHROOM, BUTTERNUT DHAL WITH FRIED EGG AND CORIANDER	.5667055	.5609917	101.0185	Vegetarian	Dark-Green
5	VEGGIE MINCE COTTAGE PIE	.2835	.22	128.8636	Vegetarian	Dark-Green
6	QUINOA WITH ROASTED VEGETABLES & BASIL OIL	.336872	.22365	150.6246	Vegan	Light-Green
7	CHICKPEA & CAULIFLOWER TAGINE WITH BRAISED RICE	.38552	.25	154.208	Vegan	Light-Green
8	SPICY BEAN BURGER IN CIABATTA ROLL	.3203418	.1775	180.4743	Vegan	Light-Green
9	MOZZARELLA, BEAN & JALAPENO BURGER	.2167962	.0981598	220.8606	Vegetarian	Light-Green
10	PLAICE FILLET WITH PAK CHOI & SWEET CHILLI	.5836047	.2475821	235.7217	Fish	Light-Green
11	GOLDEN SQUASH, PEPPER AND TOMATO GRATIN	1.161266	.4432917	261.9644	Vegan	Yellow
12	TUNA STEAK & GREEN BEAN & SESAME SALAD	.6282	.21	299.1429	Fish	Yellow
13	QUORN & VEGETABLE PAELLA	.5861453	.1845445	317.6173	Vegetarian	Yellow
14	SEA TROUT, CLAM CHOWDER AND SMOKED PANCETTA	1.765176	.4882764	361.5115	Fish	Yellow
15	VEGETABLE & NUT ROAST WITH GOATS CHEESE	1.00475	.25	401.9	Vegetarian	Yellow
16	GOATS CHEESE & HONEY TWISTS	.8570563	.16341	524.4822	Vegetarian	Orange
17	CHICKEN CHASSUER	1.7319	.306	565.9804	Poultry	Orange
18	GRILLED MISO SALMON WITH KOMBU RICE NOODLES	1.10566	.1648681	670.6327	Fish	Orange
19	PAN ROASTED SALMON WITH 3 TOMATOES	2.106173	.28738	732.8878	Fish	Orange
20	BAKED HAM WITH APRICOT & APPLE GLAZE	3.527614	.4640909	760.1126	Pork	Orange
21	ROAST STUFFED CHICKEN WITH FORCE MEAT STUFFING	1.318214	.1594	826.9849	Poultry	Red
22	PORK ESCALOPE WITH PEPPERCORN SAUCE	2.237113	.26075	857.9534	Pork	Red
23	BROCCOLI & STILTON QUICHE	2.183461	.245075	890.936	Vegetarian	Red
24	CUBERLAND SAUSAGE RING & ONION RINGS	1.9296	.21	918.8571	Pork	Red
25	ROAST LEG OF LAMB WITH MINT SAUCE	4.748612	.1574778	3015.416	Ruminant	Red

Figure 4.E3: Sub-sample of recipes with calculated carbon footprints

Note: `ghg` refers to the carbon footprint per portion and `qty` is the portion size (both in kilograms). `ghg100` is the carbon footprint per 100g serving (in grams).

Concluding Remarks

Climate change is a transformational challenge with clear social and behavioural underpinnings. Tackling the climate crisis thus requires a holistic understanding of human behaviour and its interrelations with the environment. The rapidly emerging field of behavioural environmental economics seeks to address pressing environmental issues by embracing more realistic models of human behaviour in environmental contexts, as well as experimental methods in order to inform environmental policies. Despite a recent surge in empirical research in policy relevant domains, there remain substantial gaps in our understanding of how human behaviour *is shaped* by a changing climate and associated impacts, and how human behaviour can be *actively shaped* by targeted policy interventions to accelerate a transition towards a more sustainable future.

The primary objective of this thesis is to provide causal insights into some of the complex relationships between human behaviour and the environment. It addresses specific themes that have not received sufficient attention in the academic literature but are highly pertinent to current policy debates. This thesis is a truly interdisciplinary endeavour, combining insights from psychology and behavioural economics while attaining high standards for methodological rigour, transparency, and reproducibility.

In Chapter 1, we show that exposure to air pollution has immediate and significant negative effects on mood and mental health, yet not enough to significantly impact social behaviour or economic decision-making. We argue that understanding the fundamental interactions between decision-making and air pollution requires a greater focus on strengthening experimental identification using randomised research designs and thereby providing insights into the causal relationships. To that end, we develop a novel online lab-in-the-field research design for future work to build on.

Chapter 2 shows that personally experiencing extreme weather events reduces the psychological distance associated with climate change in the UK. We show that, as heatwaves become increasingly frequent in the UK, personally experiencing such events may significantly increase concern around climate change and induce pro-environmental behaviour change. On average, however, flooding and heatwave exposure is unlikely to have far-reaching impacts on beliefs and behaviour, other than raising risk perceptions. To that end, we argue that extreme weather events in the UK could provide a “window of opportunity” for communication campaigns to raise awareness and garner support for mitigation policies amongst the general public.

In Chapter 3, the results of a large-scale online message framing experiment suggest that appealing to warm-glow motives was not successful in harnessing intrinsic motivation towards the environment. Moreover, neither negative emotive framing nor social norm framing had an effect on incentive-compatible pro-environmental behaviour. The chapter concludes that further research is required to fully understand the intrinsic motivational basis of pro-environmental behaviour and the optimal design of climate change communication strategies.

The large-scale field experiment in Chapter 4 provides causal evidence for the efficacy of carbon footprint labels in encouraging more sustainable food choices in a cafeteria setting. We show that labels induce consumers to reduce their consumption of high-carbon meals and consume more mid-carbon impact meals. We argue that carbon footprint labels present a low-cost policy solution to leverage ’climatarian preferences’ and increased demand for more sustainable food products.

Taken together, the overarching findings from Part 1 (Chapters 1 and 2) highlight the importance of the socio-ecological context in guiding decision-making. Environmental stressors – both hidden and salient – significantly impact human well-being and beliefs. The research in Part 2 (Chapters 3 and 4) illustrates the value of different experimental methods to evaluate policy interventions and gain causal insights into their potential efficacy. The findings help us understand which interventions might (or might not) work in encouraging sustainable behaviour, and lay out promising directions for future research. To that end, academics and practitioners can benefit greatly from understanding how to harness recent shifts in public opinion and pro-environmental intrinsic motivation, in order to achieve sustained behavioural change.

In sum, with this thesis, I hope to have provided some novel and interesting insights, which may directly inform current policy debates and future research, and, at least to some extent, contribute towards the collective efforts in realising a more sustainable future for everyone.